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SIR THOMAS WRIGHTSON'S THEORY OF HEARING[1]

By E. G. Boring and E. B. Titchener

The Helmholtz theory of audition, in spite of Ebbinghaus' gallant effort to save it, seems to have sunk beyond recovery. The historian of science must always regard it with admiration, since for combined range and detail and directness of correlation it is still without a rival; and indeed, as model of what a theory 'ought' to be, it will probably figure for many years to come—with the more or less emphatic warning that it need not be believed—in textbooks of physiology and psychology. Helmholtz himself, eminent in physiology and yet more eminent in mathematics and physics, was (we used to think) a living guarantee of the rightness of his theory, a physical theory of a mechanical organ: we might mistrust his theory of vision, but we could trust the physiology and physics of the *Tonempfindungen*. Little by little, however, acceptance has given way to doubt; the modern physicist will not be pinned down to Helmholtz' 'nothing but' sympathetic vibration, and objects positively that sympathetic vibration is put out of court by the dimensions and structure of the internal ear; the

[1] An Enquiry into the Analytical Mechanism of the Internal Ear, by Sir Thomas Wrightson, Bart., Memb. Inst. C. E., with an Appendix on the Anatomy of the Parts Concerned by Arthur Keith, M. D., F. R. S. London. Macmillan & Co., Ltd.. 1918. Pp. xi.+155+Appendix (156-254)+9 Plates. See also the account in *Proc. Roy. Soc. Med.*, 12, 1919, Otology Sect., 80-94; and the discussion by Bayliss, Keith, Wrightson, Perrett and Rayleigh, *Nature*, 102, 1918, 124f., 164f., 184f., 225, 263f., 304, 325.

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modern physiologist looks in vain for Helmholtz' resonators; and the modern psychologist, oppressed by the number and complexity of his facts of observation, wishes to clean his slate of theory and to start afresh from a thoroughgoing study of the sense-organ, biological, anatomical and physiological.

Meantime, of course, theories flourish, and must be considered as they come. The theory set forth in the book before us is that of an engineer, who approaches the ear "as a piece of working machinery." First conceived as long ago as 1876, it has now been wrought out in detail; and the author has sought to clinch its application by calling in the aid of an anatomist. The combination of forces is unusual; the disregard of psychology is—to a psychologist—refreshing; and we begin to read with a hopeful curiosity. May not this frankly teleological attitude toward the sense-organ, as a machine in being, put us all, physiologists and psychologists alike, on the right track?

Whether or not this hope is eventually to be fulfilled, we soon discover that the writer has not troubled to make things easy for us. His style appears at first to be clear and simple, but in fact it is neither the one nor the other. His physical terminology, in particular, is at best misleading and at worst incorrect. The terms pressure, force, power, work, and momentum are used loosely despite their precise physical meanings;[2] and discussion that begins thus inexactly ends, as it must, in positive error.[3] Moreover, the exposition of the theory is annoyingly unsystematic. We find digressions, repetitions that are sometimes almost verbal, and a general absence of the coordination that might have been effected by literary organization or by cross-referencing. Again and again the reader fails to divine the author's purpose; when is he seeking an account of the analytical mechanics of the internal ear, and when the establishment of a theory of hearing? Much of the description belongs only to the former context, and much of the discussion only to the latter, and the resulting confusion has to be cleared up by re-reading.

The burden of comprehension is thus heavier than it need

[2] *Cf., e. g.,* the confusion of work and power, pp. 134 f. The reader will find plentiful instances of a like kind.

[3] Consider *e. g.,* the application of the energetic principle of conservation to forces which should be dealt with as equilibrated, and the consequent discovery of a supposititious residual force effective in the cochlea: p. 94, lines 9 ff.

be. We do not desire, however, to make too much of this criticism. The book is a war-book; the preface is dated February, 1917; and the writer may have had scant leisure for polishing his material. The looseness of terminology is a more serious matter. We proceed to show, in a number of instances, that it means a fatal looseness of thought.

1. In his preliminary analysis of compound wave-forms [4] Wrightson shows that, if the simple wave-forms be represented by straight lines forming a series of V's, the algebraic addition of two simple wave-forms respectively of frequency x and frequency y will give a compound wave-form, which has $(x+y)$ and also $(x—y)$ approximately equidistant crossing-points (intersections of wave-form with horizontal axis). These two periodic series show no significant interrelation. Wrightson attributes to the $(x+y)$ series the summational tone, and to the $(x—y)$ series the differential tone. The correlation is, however, unsatisfactory. It is to be presumed—and Wrightson seems to imply—that similar properties in the wave-form underlie both simple tones and combinational tones. But the tone x is heard for a wave-form that has $2x$ crossing-points (a complete wave crosses the axis twice); and a tone y is heard for a wave with $2y$ crossing-points. If $(x+y)$ be the summational tone, then it might be expected to have $2(x+y)$ crossing-points. The differential, in like manner, should be conditioned upon $2(x—y)$ crossing-points. Wrightson's $(x+y)$ and $(x—y)$ are each of them an octave too low. The $(x+y)$ crossing-points, moreover, represent an average frequency, and not a sum.

2. This discussion of triangular wave-forms is preliminary. We come presently to the compounding of tones of sine-form. Here we learn that, if we regard every crest, hollow, and crossing-point as an "impulse point," we have possessed ourselves of a key to an understanding of the ear's analysis of compound waves. More concretely: if we build up a compound wave-form by the algebraic summation of sinusoidal components, and note in the complex form the positions of all crests, hollows, and crossing-points, we discover that a great many of these critical points are separated by distances which are approximately the same as the wave-length of one component; that many other pairs of points are separated approximately by the wave-length of the other component; and that

[4] Pp. 12-17; Plate I.

still other points show separations which correspond with the summational tone or the differential tone. This discovery justifies, we are told, the initial assumption that crossing-points, crests, and hollows all function in the same way.

A little reflection, however, raises the question why Wrightson, in these graphical experiments, should wish to find the *wave-lengths* of the components reproduced in the compound. In a simple tone there are two crossing-points, one crest, and one hollow, in every single wave; four impulse points in all. A tone of x vibrations per sec. has $4x$ impulses within the second. It would seem, therefore, that Wrightson should have made his analysis of the compound wave by seeking out recurrences of the quarter wave-length rather than of the wave-length. Whether an analysis with respect to quarter-waves would fail we can not tell. The original curves in the publication of 1907,[5] which unfortunately is not at present available to us, must be analyzed again (as they were originally) by a cut-and-try method.

3. There are more difficulties of the same order, but they appear at higher levels of the argument. We must observe next a fundamental assumption of a different kind.

Wrightson, in his analysis of the hydraulics of the inner ear, ordinarily uses hydrostatic and not hydrodynamic principles. He employs the terms power, work, and momentum; but the dynamic principles that these terms imply do not dominate the discussion. There is no reference to the inertia of the moving parts, although the difference in phase of the curve of air-pressure and the curve of air-displacement[6] is a matter of inertia. Wrightson draws the analogy to the steam-engine as if the work transmitted to the hair-cells were the vehicle of sensation.[7] The engineer's interest in the steam-engine, however, lies ultimately, not in its work, but in its power. Power is rate of doing work; and both the frequency and the amplitude of a tone must be considered if mechanical power is the key to hearing. An engineer determines mean effective pressure by putting a steam-indicator on the steam-cylinder, just as Wrightson wishes might be done with the inner ear;[8] but the engineer does not then forget to measure the speed of the shaft.

The substitution of static for dynamic principles begins in Wrightson's argument when we pass from the sounding air to

[5] P. 31.
[6] P. 76.
[7] Pp. 94, 134.
[8] Pp. 1, 134 f.

the mechanical system of the middle and inner ears[9]. Wrightson shows the waves of pressure and displacement separated in the air,—a dynamical separation. He then notes that the stapes moves as the result of air-pressure upon it, and assumes that it will therefore move *with* the air-pressure.[10] In a series of hydrostatic equilibria it would so move; the position of the stapes would be a function of the pressure upon it. But hydrodynamically there would be a lag due to inertia. An imponderable stapes in air would move with the ponderable air and not with the pressure of the air. An actual stapes weighted by a mass of liquid would similarly be out of phase with air-pressure.

In another passage we read: "As liquid pressure in a closed vessel exerts itself equally in every direction the unit pressure at any one moment which enters the cochlea is not affected by the shape of the vessel."[11] This misstatement is symptomatic of the entire hydraulic account of the inner ear. The law of equal pressures is the hydrostatic law. Engineers make use in the Venturi meter of the fact that pressures are not everywhere equal within a moving liquid, and that the shape of the containing vessel determines the rate of movement and thus the pressure.

The fundamental principle of hydrostatics is the equilibration of forces. Every body is considered in equilibrium and the sum of the forces acting upon it equated to zero. This mode of working is not adequate in hydrodynamics; but it is the mode that Wrightson uses;[12] and it makes difficulty later on.

If it is argued that the masses involved are so minute that their inertia is neglible, we reply that the masses are not disappearingly small and that their rates of motion are very great. Moreover, we must remember that Wrightson's theory stands or falls with the assumption that the ordinary principles of mechanics apply.

4. If we grant the dynamical nature of the problem, we must ask why Wrightson supposes that the movement within the inner ear is a mass-displacement of the lymph and not a transmission of sound-waves, since water does in fact conduct sound. Bayliss has already raised this question.[12a] Wrightson's answer is misleading. He holds that the lymph is "(at

[9] P. 76.
[10] Fig. 30.
[11] P. 47.
[12] *Cf.* pp. 80-87, esp. 83 ff., 86; 124.
[12a] W. M. Bayliss, *Nature*, 102, 1918, 124, 263.

the minute pressures dealt with) inelastic and incompressible, and will therefore be moved instantaneously in the passages of the cochlea."[13] He quotes Helmholtz as authority.[14] He is especially insistent that a liquid is inelastic.[15]

As a matter of fact, however, liquids are not absolutely incompressible and they are highly elastic. Elasticity and compressibility are *inversely* related;[16] a liquid is highly elastic because it is relatively incompressible. The true answer to Bayliss' question has been given by Rayleigh; the cochlea is so short with respect to the wave-lengths of ordinary audible tones that there could be very little difference of phase between the pressures at the oval window and at the round window, and transmission would therefore be practically instantaneous.[17]

5. We may include as a separate item in this list the fact noted above, that the curve of displacement of the stapes can not be, as Wrightson believes,[18] synchronous with the curve of air-pressure. His assumption that the two are synchronous sets the stage for the difficulties of all the discussion that follows.[19] Here the neglect of the dynamic principle is especially unfortunate because of the critical part played by the conclusion.

6. Thus we come to the most important part of the theory, the demonstration that every complete wave of pressure in the air, consisting of a positive and a negative phase, is correlated with the two positive phases and two negative phases of a pressure-curve within the cochlea. The reader must refer to Wrightson's book for the details of the argument.[20]

If we take Wrightson at his own level of exposition, we are at once entitled to object to the development of Fig. 34. Here (by analogy to Fig. 33) "C" must be the mid-position and "B" the extreme position of the stapes. The generating time-circle is struck with "B" as a center; so that the stapes, in moving from "C" to "B," is shown as starting from rest at "C" and moving faster and faster until it reaches "B," where it changes direction and moves back on the return

[13] Pp. vii, 80, 90, 115.
[14] Notes on pp. 56, 74; p. 130; and *Nature*, 102, 184.
[15] Pp. vii, 77, 121, 128, 130.
[16] Elasticity=stress/strain=compressing force/amount of compression. The less the compression produced by a given force the greater the elasticity. *Cf.* A. Winkelmann, *Handbuch der Physik*, I, 1906, 497-503.
[17] *Nature*, 102, 304.
[18] P. 76.
[19] Pp. 76-96.
[20] Pp. 80-84, Fig. 34 and Plate V.

stroke. Such a movement of the stapes is, however, manifestly impossible. The stapes can not reverse the direction of its movement at maximal speed; it must, like any other object in pendular motion, slow down and pass through rest at the moment of change. Moreover, Wrightson elsewhere notes that it halts in the extreme position.[21] Yet Fig. 34 shows it moving over a maximal space in the last increment of time!

The remedy is simple. The time-circle of Fig. 34 must be struck with its center at "C." (Where else indeed should the center of the generating circle for simple harmonic motion be placed than at the center, "C", of the total displacement?) The stapes, like a swing, moves fastest at its mid-position "C," slows down, and reverses direction at "B." It does not, as Wrightson maintains, stop momentarily at "C" as well as at "B." What, we wonder, has led Wrightson to believe in this arrest of motion at "C"? The examples of reciprocating motion with which the engineer is familiar furnish no ground for the belief. It can not arise in consequence of the invocation of Hooke's law; for it is formulated prior to the mention of Hooke's law, in order that the combined effect of Hooke's law and the twofold arrest may be worked out. Probably the error springs from the assumption that stapedial displacement is an immediate function of pressure (the fifth point mentioned above); for the pressure is zero in the mid-position, and Wrightson seems to think that the resultant velocity must also be zero. He forgets, it appears, that a swing moves fastest in its mid-position, when the effective component of gravity is zero.

If we dissent from Wrightson's reasoning, and consider the stapes as moving fastest in the mid-position, we must strike the generating circle about "C." The curve of elastic resistance, as thus developed, becomes a sine curve and not a versine. If we then take the difference between the imposed pressure (sine curve) and the opposing elastic pressure (now shown to be also a sine curve)[22] we get as the curve of transmitted pressure, not Wrightson's two positive phases followed by two negative phases,[23] but zero pressure. This is exactly the result which could have been foreseen. We are dealing implicitly with hydrostatics and forces in equilibrium. Under Hooke's law and these assumptions the elastic membranes will stretch until they give back a reacting force equal to the impressing

[21] *Cf.* pp. 87, 121, 142.
[22] *Cf.* Plate V.
[23] The red curve of Plate V.

force. Thus we demonstrate Newton's third law. Action and reaction are discovered equal!

It stands to reason that no pressure could be transmitted through the *fenestra ovalis* if there were nothing beyond to press against,—granted always that the pressures due to inertia and change of velocity are, with Wrightson, left out of account. But in point of fact there are elastic pressures arising within the inner ear. Wrightson admits them,[24] but regards them as neglible in comparison with the tensions of the tensor tympani and the stapedius. They are the elastic pressures from Reissner's membrane, the tectorial membrane, the basilar membrane, and the *fenestra rotunda*, all of which are displaced with the stapes. Far from being neglible, they are the determinants of internal pressure within the cochlea, which is of their order of magnitude. The pressure in any canal is the pressure necessary to displace the membranes lying beyond it. In the cochlear duct, the hydrostatic pressure would be atmospheric pressure *plus* the pressures due to the distension of the basilar membrane and round window. In the tympanic canal the pressure would be atmospheric pressure *plus* the pressure from the round window only. In a dynamic system the pressures would depend in addition upon the rate of motion and the inertia of the cochlear lymph. In any case, however, they would be approximately sinusoidal, and they must be accounted for by conditions within the cochlea, and not as a residual left over from the operation of the mechanism of the middle ear.[25]

7. The central feature of the Wrightson theory is this complete cessation of pressure and motion at four points within the complete period.[26] These critical points are points of zero pressure in the doubled cochlear wave, and they are correlated with the crests, hollows, and crossing-points of the air-wave. Thus we get an inkling of the significance of impulse points in Wrightson's analysis of mechanically compounded curves in the earlier part of his book.[27] We saw, however, that his analysis is faulty because the four-fold total wave, instead of the significant quarter wave-length, is laid off among the

[24] Pp. 144, 154.

[25] This whole discussion of a residual pressure which is delivered to the elastic membranes and not taken up by them, so that something remains over for further use, sounds very much as if work or energy were in question. For there is no principle of the conservation of pressure! But the main argument applies to pressure; and if Wrightson shifts his meanings without changing his words, the reader can not be held responsible.

[26] Pp. 87, 121, 124, 142, 196, 212, 218.

[27] Pp. 26-35, 60-71.

impulse points of the compound curve. We see here that the mechanical significance of the impulse points fails of establishment. We still can not deny the graphical coincidences of the curves, but we may deny that they have brought us far toward an understanding of hearing.

8. If the cochlear curve of pressure is not doubled, there is no "subjective octave."[28] "Subjectively," *i. e.* in the cochlea, there are just as many crests and troughs and crossing-points as there are "objectively" at the tympanic membrane. But even if the cochlear curve is doubled, as Wrightson would have it, we should not expect to find, as he does, that both the objective tone and its subjective octave are effective for hearing.[29] Wrightson seems to assert that for the tone m the cochlea receives impulses corresponding both with m and with $2m$. Moreover, the subject is expected to hear both m and $2m$ together; at least both m and $2m$ are held to be simultaneously effective in the production of combinational tones. This argument is untenable both in fact and in logic: one does not hear in a pure tone its octave, and one does not eat one's cake and still have it. If the cochlear determinant of tones is doubled, then it can not still be single.

9. With the subjective octave goes Wrightson's explanation of all differential tones but the first.[30] One of his differential tones, $(2m—n)$, does not exist, to our knowledge, for any other theory. The first differential is supposedly grounded in the nature of the compounded curves; but we have seen that the preliminary discussion indicated a differential tone an octave too low, and that subsequent discussion was based upon an improper unit. Hence there is as yet little promise of a revised theory of differential tones.

10. The summational tone is in no better case. Wrightson does not condition it upon the subjective octave, though he might have done so had he allowed differential tones of the second order.[31] The summational tone goes the way of the first differential tone; the arguments in its favor that we have noted in our first and second paragraphs are not convincing. Even were they final, they would prove embarrassing; since they would indicate that the summational tone is a prominent component of all fusions; whereas, introspectively, the summational tone is weak. Wrightson concludes with an attempt at

[28] P. 143.
[29] Pp. 147 f.
[30] Pp. 148 f.
[31] *Cf.* W. Wundt: *Physiol. Psych.*, i., 1893, 465.

diagrammatic establishment of the summational tone;[32] but he fails here for the reason that we have given, and for another reason that we must now mention.

11. The final account of the mode of treatment of the cochlear pressure-curves[33] raises new queries. Wrightson obtains a cochlear curve for a compound of two tones by adding algebraically the cochlear curves of both tones The result is not what we should expect (*cf.* Plate VIII); since the sum of the cochlear curves of the separate tones is not what the cochlear curve of the sum of the separate tones would seem to be. Presumably, since compounding actually occurs in the air, we should first compound the sine curves, and then find a cochlear function. Be this as it may, Wrightson now proceeds by the cut-and-try method to lay off between impulse points the wave-lengths of the separate generators, of the differential tones, and of the summational tone; and *he takes as impulse points the crests, hollows, and crossing-points of the cochlear wave.* Up to this point we had thought that the reason for calling the crests, hollows, and crossing-points of the air-wave "impulse points" is their correlation with the points of supposedly zero-pressure of the doubled cochlear wave. Now we are suddenly confronted in a simple wave with *eight* impulse points; for in a single period the cochlear wave can have four crossing-points, two hollows, and two crests. Still, however, this gratuitous doubling of the number of impulse points results in graphic concidences! It is strange that the theory, if we take the diagrams at their face value, should thus insist on proving itself. Moreover, there is something disconcerting about a theory that will come right on any assumption. We ask in bewilderment *what* it is that the theory theorizes,—whether the facts that it is supposed to be concerned with, or some irrelevant character of graphic forms.

It is a pleasure to turn from criticism to appreciation. And if the summary statement of our debt to Wrightson is shorter than the catalogue of objections to his theory, that is only because it may be set down without comment or argument. The points to which we desire to call attention lie for the most part at the level of observation,—naturally, since the conclusions at the higher level of the theory are dependent upon reasoning to which we have been obliged to take exception

1. We must note in the first place Wrightson's discovery

[32] Pp. 151 f. and Plate IX.
[33] Pp. 150-152 and Plates VIII, IX.

that the compounding of two periodic V-shaped wave-forms of frequencies x and y gives a compound curve of $(x+y)$ and of $(x—y)$ intermediate and approximately equidistant crossing-points; and that this characteristic of compounding is independent of the phase-relation of the components.[34] Wrightson apparently made his discovery by an inspection of plotted samples, and without having the ultimate form of his theory in mind. The cases presented in Plate I are convincing samples: there seems to be no reason why the validity of the generalization should be doubted. It would be interesting to work over some of the cases accurately, by analytics or graphics, in order to determine the mean variation of the points from exact periodicity and thus to arrive at a definition of the approximation.[35] The meaning of the relation, as we have seen, is not obvious; but it is a novel idea that compound waves may be understood by a study of the occurrence of critical points within them.

2. Wrightson makes a similar discovery in the compounded sine curves, plotted by the ohmograph,[36] the invention of which is in itself a contribution to acoustics. If the hollows, crests, and points of crossing are taken as critical points, the distribution of these critical points shows simple relationships to the component wave-lengths and to the wave-lengths corresponding with the sum and difference of the component frequencies.[37] Again we are reminded of the possibility that we may solve the problem of hearing by reference to a periodic recurrence of critical points, rather than by reference to the total wave-form or the wave-form of the components. Here the analogy of the siren, where the ratio of size of hole to space between holes (wave-form) is much less important than the rate of recurrence, is peculiarly suggestive.[38]

3. The conception of the conditioning of tone on the periodic recurrence of momentary impulses raises the question of the effect of aperiodic impulses. Wrightson finds critical points that are 'aperiodic,' except that they recur at long intervals; for a wave-form compounded of two commensurable components must eventually repeat itself and therefore every point within itself. He regards these additional points of long period as the correlates of noise,[39] and remarks that the

[34] Pp. 12-17 and Plate I.
[35] *Cf.* p. 30.
[36] Pp. 24 f. and Plate III.
[37] Pp. 26-30, 60-77.
[38] Pp. 6-8.
[39] P. 35.

poorer fusions, where the 'aperiodic' points occur more frequently, are the noisier. It is true that the poorer fusions have upon them a greater roughness or *vibrato*, and it is possible that Wrightson provides a key to this introspective fact. If one can hear in a major seventh two tones *and* a *vibrato*, any theory that provides three elements in the compound wave is so far illuminating. We may point out, further, that Wrightson's characterization of the 'aperiodic' recurrences as noise agrees with Jaensch's view, and that both Jaensch's and Wrightson's noises are periodic in a long period.[40]

As with the graphics of wave-form, so with the mechanics of the ear, Wrightson accomplishes more at the level of fact than at that of interpretation. His text and Keith's appendix are full of descriptive statements concerning the structure and mechanics of the middle and internal ears that are necessarily pertinent to any theory and are themselves the material of science. They can not all be listed separately, but we may indicate their general nature.

4. The kinematic analysis of the middle ear, and its conception as a mechanical system slung between opposing elastic tensions and operating under Hooke's law, at once provide valuable data regarding the actual dimensions and the probable displacements of parts.

5. The analysis of the inner ear is similarly informative. It is of exceptional value that we should know the hinge-like motion of the stapes,[41] the dimensions of the cochlea, the relative velocities of the lymph at various points, the size and relations of the various members of the organ of Corti at different regions of the cochlea, the probable displacement of the basilar membrane, the hinging of the arch of Corti, the relation of the tectorial membrane to the hairs, the kinematics of the organ of Corti that translates basilar displacement into transverse bending of the hairs,[42], the approximate amount of this displacement,[43] the elastic resistance of the tectorial membrane,[44] the lack of correspondence between the hair cells, the tectorial fibres and basilar fibres,[45] the peculiar properties of the vascular body,[46] and the other details of like

[40] Wrightson, pp. 21 f.; E. R. Jaensch, *Zeitschr. f. Sinnesphysiol.* 47, 1913, 256 ff.
[41] Pp. 42 ff.
[42] Pp. 95-127.
[43] Pp. 189 ff.
[44] P. 209.
[45] P. 247.
[46] Pp. 181 f.

nature. The study of the inner ear of the sparrow shows that certain parts of the human ear are in all probability inessential to a discriminative hearing of tones.[47] The suggestion that the broadening of the basilar membrane toward the tip of the cochlea is a compensation for its less favorable situation is also worthy of notice.

All these facts are of great value, even though they have not as yet eventuated in a tenable theory. They belong to the observational half of science, and the primary half. If we had had more of them and fewer theories, we should know more; or rather, if we had had more of them, we should have fewer theories. Wrightson has given us no new theory, that shall "win the assent and support of physiologists, psychologists, physicists and musicians;" so far as that goes, the Helmholtz theory may continue to rest as comfortably as its intrinsic condition permits. But he has given us more facts than we have had for many a day.[48] And he has thus begun his scientific work where it should be begun,—with observation.

[47] P. 228.

[48] Let it not be forgotten that Wrightson, when he lacked facts, sought out a competent preparator to make for him three series of microscopical sections of the human ear. See Appendix, p. 157.

ON THE NON-VISUAL PERCEPTION OF THE LENGTH OF LIFTED RODS[1]

By Louis B. Hoisington

TABLE OF CONTENTS

	PAGE
I. Problem	114
II. Observers	114
III. Apparatus and Procedure	115
IV. Results—General	118
Length	119
Weight	120
Center of Mass	128
Synthesis	136
V. Secondary Factors—Visual Imagery	139
Muscular, Strain and Joint Sensations	141
The Temporal Factor	142
Pressure Gradient	143
Absolute Length Judgments	143
VI. On the Computation of a Limen	144
VII. On the Application of Weber's Law	146
Conclusions	146

I. Problem

This study has been undertaken as a preliminary to the study of what is known as 'eccentric projection.' We have sought to discover and to analyze the perception of length which arises when light rods, held horizontally, are lifted a moderate distance in the vertical dimension. We have also been successful in synthetizing this perception of length from the data of analysis. We believe that the ground is thus prepared, logically and psychologically, for an attack upon the more complex perception which appears when a rod, held in the hand, is brought into contact with wall or floor.[2]

II. Observers

The observers were Mr. P. Cavanaugh (Ca), Miss C. Comstock (Co), Dr. C. L. Friedline (F), Dr. J. M. Gleason (G), the writer (H), Mr.

[1] From the Psychological Laboratory of Cornell University.

[2] Since the references to this type of experience are concerned almost exclusively with 'eccentric projection,' we omit an historical review. See, however, H. Lotze, *Microcosmus* (tr. E. Hamilton and E. E. C. Jones), I, 1885, 586 ff.; esp. on the perception of added height due to head and foot coverings, 592 f.

H. S. Liddell (L), Mr. E. S. Neal (N), Miss A. K. Sullivan (S), and Dr. M. J. Zigler (Z).[3] All were graduate students or instructors in the department of psychology. They varied considerably in training: Ca and N were unpractised; Co, L, S and Z were but moderately experienced; F, G and H were trained observers.

H and Z served throughout; F, G and N during the first half of the work; L and S during the second half; Ca and Co during the last quarter only. The observers, except H, worked without knowledge of problem or conditions. The plan of the investigation called for observation under a long series of but slightly varying conditions, and thus made heavy demands upon the patience and skill of the observers. The demands, as the results show, were adequately met.

III. Procedure and Apparatus

The method of procedure was twofold: quantitative, by the method of Constant Stimulus Differences, and qualitative, by way of introspective report. In the method of constant stimulus differences, following Urban, we used five comparative stimuli: objective equality, and two steps above and below; except in one check-series, where all rods were objectively equal.

The experimental work divides itself into four principal sections—A, B, C and F—in accordance with the nature of the rods used or the end sought. In section A (10 series) we used rods, planed from seasoned poplar wood, one-half inch in diameter, which averaged 1 gr. in weight per 2.85 cm. of length. The handles, turned from white pine,, were quite heavy relatively to the weight of the rods. In section B (7 series) the rods, planed from seasoned oak, were three-quarters of an inch in diameter, and averaged 1 gr. in weight to every 0.52 cm. of length. The handles, also turned from white pine, were both absolutely and relatively much lighter than those used in section A. In section C (1 series) we aroused the experience of length without recourse to rods, synthetically. In section F (6 series) we sought to determine more exactly the effective factors in the perception of the length of lifted rods. To this end we used the lighter rods with a new set of lighter handles. We added weights to the rods, to make them conform to certain mathematical requirements; hence they were about double the weight of the rods of section A. We give the data for the physical properties of the rods in Table I.[4]

This table shows three characteristic ways in which rods may differ: length, weight and center of mass. All three moments may differ for every rod in a set; or any one moment may be kept constant for every rod, the other two varying; or two may be constant for every rod in the set, the other varying. We thus obtain seven different combinations. Use of the same combinations with the light and the heavy rods gave fourteen series; the first seven of section A and all

[3] Thanks are due to Miss M. Kincaid and to Dr. C. W. Bock, who acted as experimenters for H.

[4] The handles for section A were 15.4 cm. long and weighed 70 gr.; for sections B and D, they were 15 cm. long and weighed 30 gr.; for sections E and F they were 10 cm. long and weighed 30 gr. The centers of mass in series A III lay 35, 33.5, 31, 29.5, and 27 mm. back from the front end of the handle.

of section B.[5] In addition, we ran a control-series (A VIII), in which all the rods were equal; a practice-series (A IX), which was

TABLE I.

Series	Total Length of Rod and Handle	Center of Mass from Handle	Weight of Rod and Handle
A I	74, 79.7, 85.4, 91.1, 96.8 cm.	11, 23, 35, 47, 59 mm.	96, 98, 100, 102, 104 gr.
A II	Same as A I.	Same as A I.	All 104 gr.
A III	All 22.9 cm.	See foot note	Same as A I.
A IV	Same as A I.	All 59 mm.	Same as A I.
A V	Same as A I.	All 59 mm.	All 104 gr.
A VI	All 85.5 cm.	Same as A I.	All 100 gr.
A VII	All 85.5 cm.	All 35 mm.	Same as A I.
A IX	Same as A I.	Same as A I.	Same as A I.
A X	Same as A I.	Same as A I.	Same as A I.
B I	85, 87, 89, 91, 93 cm.	223, 233, 244. 255, 266 mm.	193, 196, 200, 204, 208 gr.
B II	Same as B I.	Same as B I	All 208 gr.
B IIIa	All 25.5 cm.	21, 23, 25, 27, 29 mm.	Same as B I.
B IIIb	All 89 cm.	Same as B I.	Same as B I.
B IV	Same as B I.	All 244 mm.	Same as B I.
B V	Same as B I.	All 244 mm.	All 208 gr.
B VI	All 89 cm.	Same as B I.	All 200 gr.
B VII	All 89 cm.	All 244 mm.	Same as B I.
C I	Synthetic Series.		
D I	Same as B I in all respects. Wt. judgments.		
E I	All 82.5 cm.	All 250 mm.	60, 80, 100, 120, 140 gr.
F I	All 82.5 cm.	74, 76.9, 80, 83.2, 86.7 mm.	208, 204, 200, 196, 192 gr.
F II	All 82.5 cm.	206, 222, 240, 261, 284 mm.	111.6, 105.85, 100, 94, 87.9
F III	All 82.5 cm.	Same as F II.	116, 108, 100, 92.2, 87.9 gr.
F IV	All 82.5 cm.	202, 220, 240, 262, 285 mm.	All 100 gr.
F V	All 82.5 cm.	Same as F IV.	113.5, 106.6, 100, 94, 87.6
F VI	All 82.5 cm.	Same as F IV.	119, 109, 100, 91.7, 84.2 gr.

[5]The 7 series of section B duplicate the corresponding series of section A in the ordering of the three physical moments.

a repetition of A I, at the end of the year's work; a passive series (A X), in which the support of the rod, after *O* had grasped the handle loosely, fell away gently; the synthetic series (C I); a weight-series, in which the *O*'s judged the comparative weights of the rods; two short series in which a wooden 'spoon,' securely bound to the arm, served as artificial thumb or forefinger to take up the 'kick' or the 'pressure' as the case demanded; and a series (E I) in which center of mass and length were the same for every rod, but weight varied very considerably. Lastly, we ran a somewhat irregular lot of series (Section F) in the hope, as before stated, of finding some further cue to the analysis of the perception. We worked out the weight and the center of mass that would give us a desired ratio of 'kick' to 'pressure,' and increased this ratio by relatively equal amounts for every one of the five variable rods (F I and II). In this way we hoped to find an optimal setting of the rods—i. e., that combination of physical moments which should give the smallest D. L., or whatever corresponds with that determination. We also (F III, V and VI) placed weights on the rods at such points that the 'kick' or the 'pressure' remained constant for every rod in the set.[6] In F III the steps were relatively equal increments of the variable pressure. Finally (F IV, V and VI) we took for our steps relatively equal increments of center of mass. In sections A, B, D and E the steps were absolutely equal units of weight, of length or of center of mass. There were, then, one, two or three sets of equal but disparate increments, acocrding to the number of variable factors for the given set of rods.

In an experimental group 20 pairs of rods were presented in a haphazard order, in which every one of the variables occurred an equal number of times. On the completion of a group, *O* rested for a few minutes. The number of groups completed in an hour of observation varied from three to five. Fifty judgments in all were made with every variable in every series, except the first in A and B, when for the sake of practice the number was increased to 100.

The time elapsing between presentations of the rods was, at first, regulated by a soundless metronome. The use of the metronome was discontinued after *E* had become practised. Six seconds intervened between the giving of the judgment and the presentation of the first rod of the next pair. We found that, with this interval, the tendency for the impression of the last rod of a preceding pair to carry over to the first of the succeeding pair was almost negligible. The second rod was taken up by *O* as soon as he was able after putting the first rod down. The time elapsing between the lifting of the second rod and the giving of the judgment varied considerably. The *O*'s were, of course, urged to give immediate judgments; if they reported an attitude of doubt or uncertainty, the pair was presented again at a later time. The time for lifting the rods and making the judgment, when the report was immediately rendered, averaged about 10 sec. Some *O*'s, however, were slow and deliberate in the act of lifting, while others made the lift very quickly.

We employed the two time-orders with equal frequency but in no regular sequence, so that any effects of warming-up or of practice

[6] Throughout this study we use the term 'kick' to designate the pressure-sensation at the base of the thumb stimulated by the up-thrust of the back end of the handle, and the term 'pressure' to designate the sensation in the forefinger stimulated by the down-pull of the outer end of the rod.

were equally distributed. *O* was not informed as to the time-order in any series. We found that, so long as he did not take a positive set or attitude toward time-order, there was complete uniformity in results. Some *O*'s showed a tendency to judge the second rod as 'longer' or as 'shorter' with a resulting difference in distribution for the two time-orders.[7] The results given are the averages for the two time-orders.

Since it was advisable that *O* should take all rods from the same place, we made a long narrow frame which slid back and forth with but little noise over round metal bearings and between greased guides, and placed it on a low table. We cut shallow notches with sloping sides 6 in. apart in the sidepieces of the frame, and faced the top of the notches with thin felt. The rods, placed in the notches, lay across the frame in the horizontal position with the handles projecting over the edge of the table toward *O*. Thus *O* took all rods from the same position, relatively to himself, since a small movement of the frame, made by *E*, brought the desired rod directly in front of him. We were able by means of this device to avoid a space-error.

The *O*'s sat blindfold during the observations. A counterbalanced screen left only the handles of the rods exposed. This screen remained down, so that the rods were completely covered, whenever the blind was removed. *E* marked the position of the chair for every *O*, so that he should sit, always with the chair in the same position, at full arm's length from the handles. *O* deposited every rod, as lifted, in a cloth-lined tray placed just above and to the right of the rod when in position to be taken up. This tray was shorter than the rods, except for series A III and B IIIa; hence *O* could get no cue to length when he put the rod down.

For the regular quantitative series the written instructions handed to the observers were: "You will be given two rods in succession. You are to raise them in the horizontal position, using a whole-arm movement and a loose grip upon the handle. You are to judge the length of the second rod in terms of the first; that is, you are to judge the second rod as longer than, equal to, or shorter than the first." For the introspective reports we took separate short series at the end of the regular groups. The additional instructions ran as follows: "You will make a full psychological report of the processes set up in the two experiences."

E's task was threefold: he shifted the sliding frame back and forth; he returned the rods from the tray to the frame; and he recorded the judgments and reports of *O*.

IV. Results

Our criterion for the adequacy of the perception of length is the ability to make comparative judgments. If the percep-

[7] On two different days, owing to a previous discussion with *E*, H assumed, without intending to do so, a time-order set. The results were comparable with those on other days if the set corresponded with the time-order given; if not, the results showed the discrepancy. In both cases *O* took the normal discriminative length-attitude at the next sitting. Following accepted practice, it would seem allowable, if *O* tends to note the time-order used, to tell him what the order is. But it appears best to say nothing about it, and to hold *O* to the attitude set up by the instructions.

tion changes in degree with variation of one or more of the physical moments of the stimuli, we shall expect a high percentage of 'shorter' judgments at the one extreme and a small percentage at the other, provided that the extremes have been well chosen. The opposite will be true of the 'longer' judgments. If, on the other hand, certain of the physical moments are not adequate stimuli to the perception, or affect it only indirectly through association and reflection, then, with these moments the sole variables, we shall expect a more irregular distribution of the comparative judgments; one with low extremes for either kind of judgment and with inversions, one with a probable excess of the 'equal' judgment. This is what we found. A certain factor (center of mass), when varied, gave typical cross-curves with low intersections; the other factors (length and weight) as variables gave flat curves with inversions and delayed responses.

The distributions of the comparative judgments for all *O*'s with the different arrangements of the physical moments (see Table I) are given in Figs. 1 to 26, where the abscissae represent the five variables, and the ordinates show the percentage that the number of judgments in each category (longer and shorter) formed of the total number of judgments given.

Length

It is clear that length, in and of itself, cannot affect the rod as a stimulus-object. It is impossible that length alone, apart from its usual connection with center of mass and weight, can affect a rod which functions as a lever. Center of mass and weight are the factors that enter into the computation of the moments of force. The pendular period of a rod is likewise dependent upon its center of mass. It is only in so far as air resists the motion of a rod that greater or lesser length, presenting a more or less extended surface, can be effective in experience. And it is out of the question that such a difference should be effective in the present experiment. In a trial synthetizing apparatus we used, without effect, a rod considerably longer than any of those of the regular series.

We, nevertheless, let length take its turn, along with the other two moments, at varying and remaining constant. The results (Figs. 5 and 15) show that, when length is the only variable, accurate judgments of difference in the length of the rods are impossible. The curves for all *O*'s are flat and irregular. These results should be compared with those obtained when all the rods in the set were equal (Fig. 8).

It must be remembered that, since weight and a center of mass out beyond the hand were present, the perception of length as such was possible and took place. The introspective reports can give us nothing new on this point.

Weight

Since we have ruled out length as a contributing factor to the length-perception, we are free to examine the series in which weight and length were both variables as well as the series in which weight alone varied (see Figs. 4, 7, 14 and 17). Scrutiny of these series shows that the influence of differences of weight upon judgments of comparative length is a complicated matter.

The adjustment of the physical moments in series A IV (Fig. 4) gave a rather large per cent. of 'shorter' judgments for the lightest rod; the per cents. for Z and N are 66 and 65 respectively. All *O*'s except F gave fewer judgments of 'longer' for this series when comparing the heaviest rod with the normal; F gave the same number for 'shorter' and 'longer' at the extremes.[8] The general flatness of the curves together with the inversions shows that here was no sufficient basis for judgments of comparative length. The preponderance of the judgment 'shorter' seemed to have, in large part, a temporal basis. Two time-factors were noted by the *O*'s; the one had to do with the quickness or slowness with which the pressure came on initially, the other, with the 'liveliness' or 'deadness' of the pressure. It was the former, according to the introspections, which came in most frequently in this series. The usual muscular innervation was greater than that required for lifting the light rod. As a consequence the rod came up with surprising suddenness. In almost every such case *O* judged the rod as shorter than the standard. The lightness of the rod came, readily enough, to carry an assigned meaning of length, in the same way that the 'secondary criteria' carry meanings in the domain of visual space-perception. It is to be doubted, however, whether the length-meaning was fully realized. It is much more probable that weight was the first item of perception immediately translated into length-terms; it set off the length-meaning without the full perception of length.

We turn now to the results of series A VII (Fig. 7), where there obtains a greater practice effect (for the effect of prac-

[8] Owing to a previous injury to the hand and wrist, F lifted the rods in a different manner from the other *O*'s. Her experience differed accordingly. Wrist-torsion made up a large part of the experience.

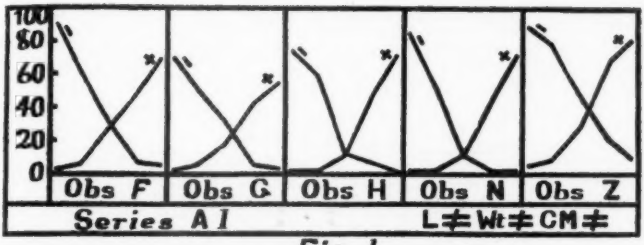


Fig 1

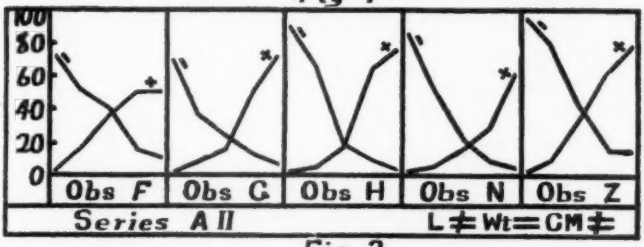
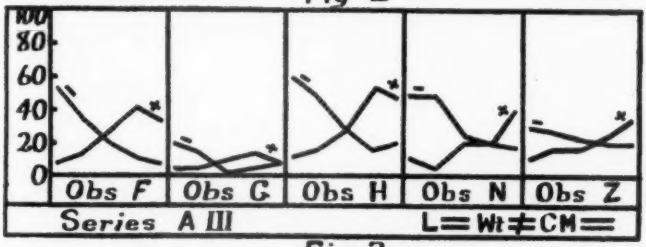


Fig 2



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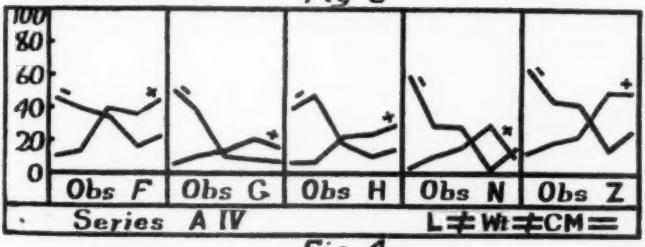


Fig 4

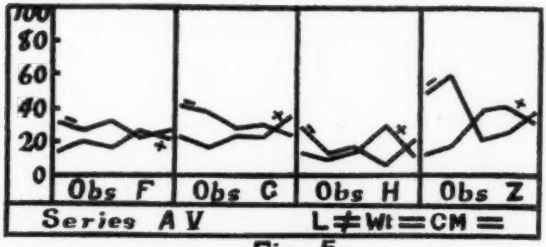


Fig 5

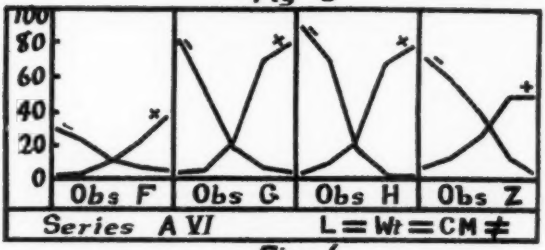


Fig 6

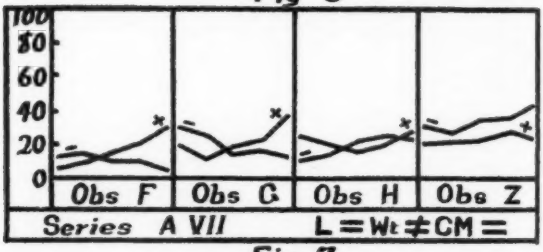
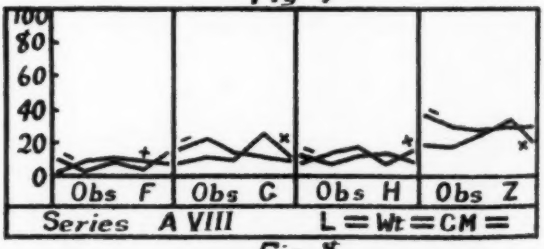


Fig 7



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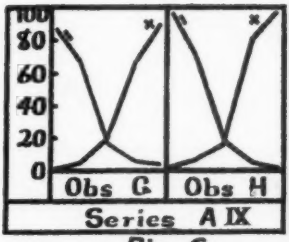

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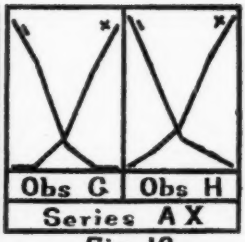

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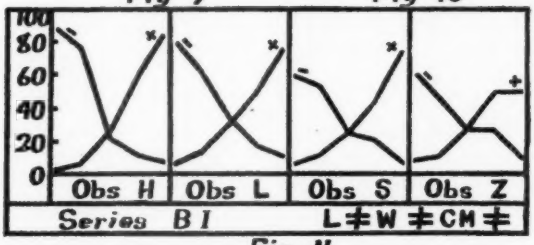

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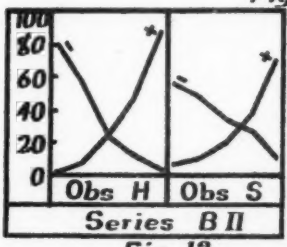

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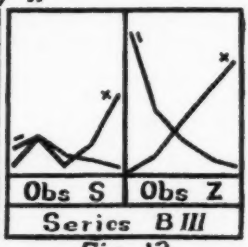

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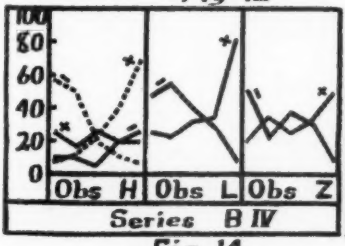

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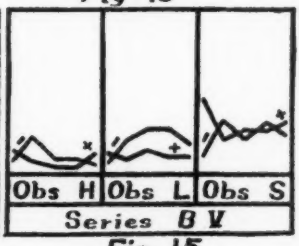


Fig 15

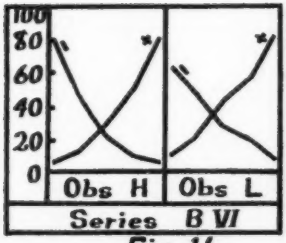
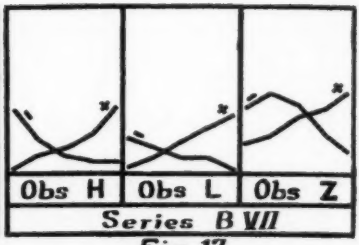


Fig 16



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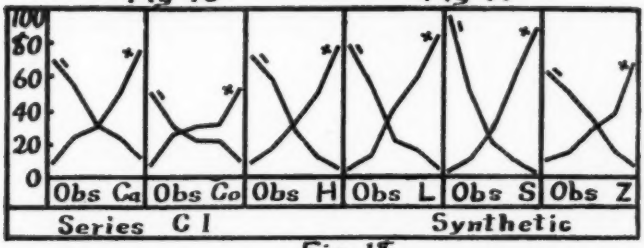

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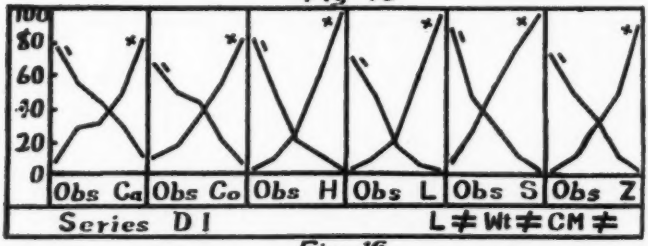

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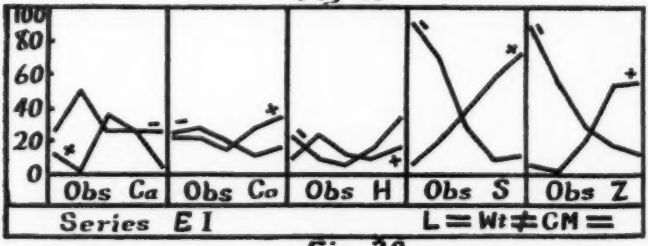

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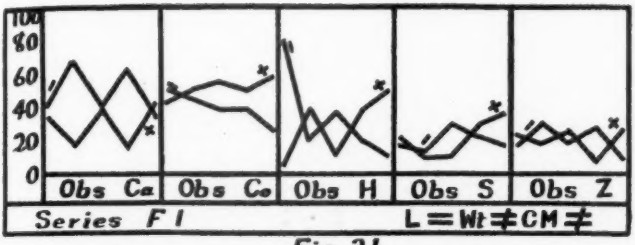

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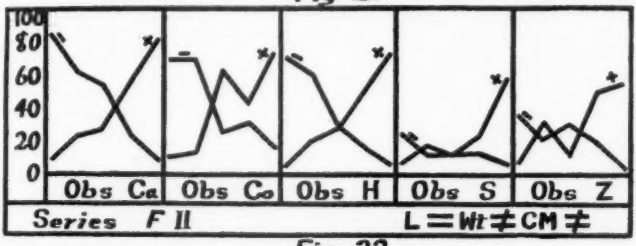

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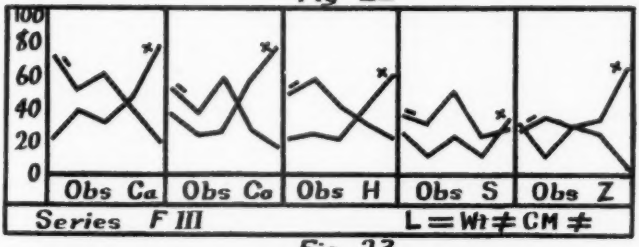

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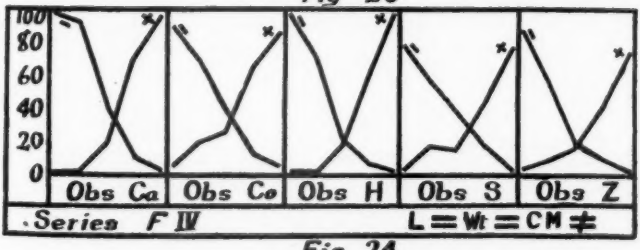

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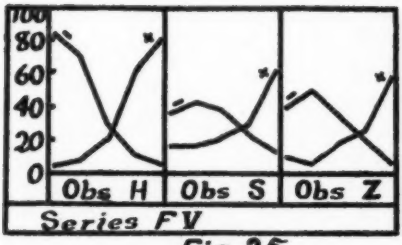

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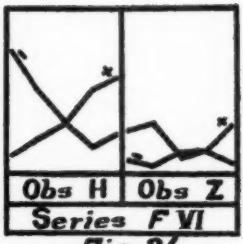

Fig 26

tice cf. Figs. 1 and 9) and where weight is the only incremented factor. The curves are flat; and there are inversions of the first order in every one save the curve for the 'longer' judgments given by F. It is obvious that there were, in this series, no sufficient sensory data for the differentiation of perceptions of length; the weight-increments, which were the same as those used in the preceding series, were not vital for the experience. Hence they cannot be held responsible for the results obtained earlier.

In spite of these curves, however, we believe that it would be a mistake summarily to dismiss the moment of weight as bearing no relation to length non-visually perceived. In Fig. 14, with weight and length varying, and in Fig. 17, with weight alone receiving an increment and a decrement, we have curves showing a tendency toward correlation between objective weight and perception of length; the lighter rods come to the *O*'s as 'short,' the heavy as 'long.' The tendency is especially striking for H in both series and for L with the heaviest rod in series B IV.[9]

This relationship finds its explanation in a gradual shift of attitude on the part of the *O*'s. They came, by degrees, to observe a single aspect, the intensive change, in the total complex (since this was the only variable present), and to report differences of intensity as differences of length. They were, naturally, determined in the direction of finding a difference; the judgment 'equal' did not mean, in many cases, the perception of equality, but the perception of not-different. It may be, too, that the introduction of the introspective series, necessitating the direction of attention upon the sensory experience, made for the translation of sensory cues into the meaning of length. We believe that, whether these series had been introduced or not, the *O*'s would have sought out some sensory cue within the complex as a means for making comparisons. Such a cue could never be adequate to the full perception of length, since this has as its primary condition not a simple attribute, inhering in a single sensation, but a ratio of intensities.

In this dilemma of having to make a judgment of difference, under the determination for differences of length, upon an inadequate sensory basis, the *O*'s seized upon the outstanding feature in the shifting complex mass, and forced that into service. Instead of apprehending the two intensities in rela-

[9]The two curves shown for H in Fig. 14 represent an earlier (dotted line) and later (full line) performance of these experiments. The change of result with shift of attitude is noteworthy.

tion, which is impossible except as they integrate in the perception of length, the *O*'s took the intensity of the 'kick' or of the 'pressure,' absolutely or in relation, as immediate data for judgment; the intensity of either or both of these pressures is affected by a change in weight as well as by a change in center of mass. The above results, accordingly, find an explanation which at the same time makes clear the true part played by weight in the judgment of length.

This shift of attitude is precisely what we might expect to take place; it is in keeping with all that we know concerning the 'natural' life-history of perceptions. The tendency is that, sooner or later, some aspect of a complex experience comes to stand for the whole; that a fragment, tag or remnant of the full sensory complement is effective to arouse and carry the meaning, fully maintained, that once accrued to the total process-complex.

But there is other evidence against the case for weight. In the experiments in which artificial wooden 'spoons' were used to receive the 'kick' in the one case and the 'pressure' in the other, the differences in the weights of the rods were not sufficient to call forth differences in the perception of length; in fact, the perception of length did not appear at all. And in series A III, where the center of mass was very near the forefinger (15-25 mm.) and varied by increments of only 1.5 mm., the perception of length was frequently lacking; the distributions of 'shorter' and 'longer' judgments (Fig. 3) shows flat curves with inversions of the first order. The more regular distributions of F and H are explicable.

F's once injured wrist necessitated a peculiar manner of taking the rods, and a dependence upon wrist-torsion for making the judgments. With a rod set as a lever in the hand, where the forefinger and the base of the thumb act as fulcrum and weight respectively, the weight or force is adequate to produce a torque in the wrist. A failure of F to render judgments of difference in this case would have proved perplexing; just as her objectively poorer results in A II were to be expected. H, unable for the most part to get the perception of length and, when it was present, unable to make judgments of difference, resorted to an individual way of taking the handles. Instead of employing the whole hand, he took the handle in such a way that in front it stimulated the forefinger alone, and at the back a very small area at the base of the thumb. By thus limiting the stimulated parts with the 'kick' and the 'pressure' (so to say) concentrated, he was enabled to make judgments. It is clear that one of two things

must have happened; either H singled out for observation the intensive changes of a single sensation; or the enhancing effect of the smaller area made smaller total-complex differences capable of carrying meanings otherwise unattainable. The former alternative accords better with the known facts. Although, therefore, H was set to report differences of length, yet by a process of substitution the inequality of intensity of the single sensation in the total complex came to touch off, directly enough for judgment, the meaning of difference of length.

Finally, series F I, F II, F III, F V and F VI demand a word of explanation. In these series the heaviest rod had its center of mass least far out and conversely, so that the two factors, weight and center of mass, ran counter to each other; they stood in an inverse ratio for the five variable rods taken in order. This result was secured by an addition to, and shifting of, the weight of the rods, in order to make them conform to certain requirements. In each one of these series either the 'kick' or the 'pressure' was the same for every rod, while the other factor increased in amount from rod number one to rod number five; the increments in series F I and F II yielded relatively equal decrements in the ratio of 'kick' to 'pressure;' in series F III gave relatively equal increments in the pressure itself; and in series F V and F VI gave relatively equal increments between the center of mass and the hand.[10] The results show marked differences between *O*'s: for some, the discrepancies of weight and center of mass cut across one another, so that the effect of the one practically nullified that of the other; for others, the differences of weight determined the judgments of length; for still others, the judgments of length reflected but slightly the opposing differences of weight. The same contradiction in results came out in series E, where we have very large increments of weight but equal centers of mass. Co and H gave hardly anything but equal judgments; S and Z gave judgments which correlate highly with weight. The fact that the judgments of two *O*'s do not correlate with weight is conclusive; the two perceptions are of different kind and, under constancy of determination, the one modifies the other but slightly even though they are at variance. The difference is that between an experience in

[10]In making our computations, we disregarded the facts that the pressure falls upon an area and not upon a point, and that the difference in the size of the hands of the observers somewhat changed the absolute values.

which a cue touches off the meaning and one in which a perception comes to full realization.

The following reports[11] taken from a much larger number illustrate *O*'s experience when set to judge differences of length by stimuli varying either in weight only or in weight and length. They supply twofold evidence for the position that differences of weight gave the perception of differences of intensity, and did not give rise to a true perception of difference of length. They show, on the one hand, that differences of weight were noted with contrary judgments of length; and, on the other hand, that differential judgments of length were touched off by differences of perceived weight,—the point being that the *O*'s could say that the judgment was one of difference of weight and not one of difference of length. They also afford proof of the statement that the arrangement of pressures within the pattern was identical for rods of different weight, the only difference being that of absolute intensity. The *O*'s evidently took the attitude of reporting the experiences upon which they based their judgment, although the instructions do not call for such an attitude. The reports are not, in all cases, rendered in strictly attributive terms; there can, however, be no mistaking their import.

Series A III: (3—5+). "There was present this time the notion of something out there beyond the handle. Can make weight-judgment always. The difference from stimulus to stimulus seems to be one of degree rather than one of pattern. This time a foundationless inference on basis of weight" (G). (3—5=) "Perception of greater weight with the second rather obscure. The clear thing was the pressure distribution in the hand, which was the same in both cases" (G). (5—3=) "Pressure-pattern same in both cases. Unequal intensity of pressure-sensations from front and back of hand give idea of length" (N). (3—1=) "First heavier. Idea of length from pressure on back of hand and pull on fingers. Second about the same degree of unbalance but lesser weight. The relation of the back and front pressures was the same in both cases, although the intensity was less in the second for both pressures" (N). (3—4+) "Greater pull in

[11]The experimental series from which the reports are taken is given at the top of each group. The numbers before the quotations indicate the rods used for comparison. For convenience, the shortest rod, or the one with the nearest center of mass or, in case they are of the same length and have the same center of mass, the lightest rod is designated as rod number one, the others coming in ascending order. Rod number three is equal, physically, to the standard. From these numbers and the table of physical moments, it is easy to determine the physical relations between any two rods involved in the reported comparison, since the order of succession in that table is the same. The judgment rendered in every case is shown by the sign +, =or—; + standing for longer, = for equal, — for shorter. These legends are used throughout.

wrist and greater pressure on fingers. End of handle presses up against palm of hand and sets up pull in wrist" (F). (5—3—) "First was heavier; more intense pressure on fingers. Heaviness gave length. Pull on end gave length" (F). (3—1=) "Pressure on fingers and pull on wrist same" (F).

It is to be noted that in these last judgments and in all others of the same kind *O* based his judgment upon the absolute intensity of a single pressure, or of both pressures, independently of their relation.

(3—5—) "I first perceived weight. Sole cue for length was pressure sensations of hand, especially forefinger. Aware of weight from muscular sensations. Attention to sensations from hand gave definite length. With second rod, I was again aware of heaviness at the end" (Z). (3—5+) "Sometimes there is intervention of the weight-factor. I am apt to assign greater length to heavier rods. By closer concentration on the pressures in the hand and especially on the fingers I can usually, on the basis of this criterion, judge the length and hence differentiate between the two factors" (Z). (3—2+) "With first, just bare pressure-complex carried over in memory-after-image. There was a little throat-kinaesthesis that meant medium. With the second experience, the pressure was the clearest part. It was perceived both qualitatively and quantitatively, but the quantitative aspect was prominent and set off the reaction *longer*" (H).

Here again we see the influence of the intensive factor taken in an absolute way. The same thing is true of the next record. We found, in support of Meumann on the comparison of time intervals, that the pattern of consciousness with the comparison stimulus differed in certain essential respects from the pattern with the normal stimulus. The latter usually resulted in a full realization of the perception of length; all the sensory data came forward; the former resulted in a very scanty perception of length, a bare meaning of more or less; the apparatus, set for giving length, responded to any cue.

(3—3+) "Pressure complex the clear thing in the first experience. With the second, the pressure at the two points was immediately perceived as greater and the response came" (H).

Series A IV: (3—1=) "I got length and a notion of rod from both. Second was heavier. Both easy balance. The pattern was the same but pressure more intense in the second" (G). (3—3=) "Got the notion of long rod from the first. Fluctuating pressure. Second less fluctuating but more intense. The two sorts of criteria give the same notion if one is not critical" (G). (1—3=) "No difference in unbalance but the second was heavier" (N). (3—5=) "First was slightly heavier. A definite idea that length was there, but can't give a comparative judgment" (N). (3—5+) "Little heavier pressure on fingers and pull in wrist a little greater with the second" (F). (3—2=) "Pressure on fingers and pull in wrist the same" (F). (3—4=) "With the first rod, I was first aware of a certain weight. Then attention was drawn to the sensations in the hand and I got a definite notion of length. It appears that I am always baffled by the determination of length in distinction to that of weight. If I do not take care, I confuse a heavier rod as being longer. Both weight and

length seemed to be determined by pressures in hand" (Z). (3—2=) "The first gave live contact-pressure with deeper dull pressure in forefinger; lively pressure of less intensity on base of thumb. Second experience less intense all round. Perception of length the same" (H).

The next reports also show both that there is a tendency for judgments of difference of length to be touched off by differences of absolute intensity, and also just the contrary: that differences of absolute intensity are ineffective to qualify the perception of length. As we have already remarked, the instruction to report differences of length, even when there were none, laid a heavy burden upon the *O*'s.

Series A VII: (3—1=) "Barest experiences. Just the same. Perception of an evenly balanced rod. When I report 'same' it is the sensory experience and not length that is the basis of judgment" (G). (3—3=) "With the first, a notion of length. Spatial pressure pattern. Second gave notion of heavy rod. Pressure perceived as weight and not as length" (G). (3—5=) "Nothing there to enable me to make the comparative length-judgment. Both uniform, static, dead. Second more intense, but did not carry meaning of length" (G). (3—2=) "With the first rod I had an immediate perception of length. This was clear. I got nothing, at first, with the second but weight" (Z). (3—1—) "First rod gave an immediate clear notion of length. With the second rod I was at first struck by considerably greater weight. Then in verbal-motor terms; 'which is longer?' The extent was limited by the points of weight-concentration" (Z). (3—5=) "The pressure-patterns were the same quantitatively and qualitatively. The second was perceived as equal, the judgment being part of the perception" (H). (3—2=) "First pressure-complex in the hand gave clear perception of length, a moderately long rod. With the second experience the pressure meant weight. Pressure more intense and static in quality. There was a tendency to say shorter" (H).

In series B III, where we used rods whose lengths, measured in terms of center of mass, were in the approximate region of the lower limen for the non-visual perception of length, but whose weight-increments were the same as in preceding series, we obtained more proof of the same kind.

Series B III: (3—1 No judgment) "Pressure on first finger and palm of hand in both. No muscular strain or deeper-lying sensations that I usually got. I seemed to be waiting for a pull downward on first finger; it didn't come, and I could not give a judgment of length" (S). (3—5=) "Meaning of length did not go beyond the handle. But the handle was stretched out. Pressure on finger and palm of hand and thumb. There was a strain-sensation, but it seemed as a bare weight. It came at the same time as pressure. It was more through the hand and a little in the wrist, not on forefinger" (S).

The following, also from the series in which we used the heavy rods, bring out the same points.

Series B IV: (3—2—) "The second experience was less intense and the judgment referred back to the first" (L). (3—5+) "The two experiences did not differ except on the score of intensity. I could not localize intensity or say how it was linked up with 'longer'"

(L). (3—1—) "Experiences were precisely homologous. The first was more intense than the second. This intensity seemed to insure or lead to the judgment" (L). (3—4+) "There was a clearly marked difference in intensity of pressure sensations in the hand. The greater intensity of the second did not carry with it a judgment of length difference" (L). (3—1—) "The second experience differed in absolute intensity and this intensity-difference went directly over into length" (L).

The next report, typical of a small class, indicates the effect upon the perception of length of shift of attitude. *O*, it will be remembered, was under a double determination; that of making a psychological report upon the process-contents of the experience, and that of making a perceptual judgment.

(3—5+) "The first thing with the second rod was sensations in the hand; those at the heel a little more prominent and intense. Almost immediately I noted a 'drawing' in the fore-arm which was more intense and 'drawing' than in the former case" (Z). (3—1—) "With the first rod, the sensations from the forefinger and the heel of the hand equally clear for a moment. The notion of length was very clear. With the second experience, I perceived sensations in the heel of the hand. Fairly definite notion of length. Then sensations in the arm with the verbal-motor 'heavier.' There also came a change in the perception of length. It became shorter. They were about equal before this time" (Z).

As regards these and the next following reports, it is to be borne in mind that, as is shown by the psychophysical results, the *O*'s were *not* able to make judgments of difference of length that have a significant degree of correlation with differences of weight. Failure to take the two sets of results together and to interweave them gives a false perspective.

(3—4+) "Pressure at base of thumb clearest part of experience. This 'live' pressure was more intense in the second case. The obscure dull pressure on the front finger was the same in both" (H). (3—1—) "The judgment had a purely sensory basis. The pressure complex was less intense in the second. The judgment came automatically without length being perceived" (H).

Finally, we cite a few reports from the last series, in which weight was the only incremented factor, to show that the perceptions of weight and length are differently determined, that the primary and secondary sensory processes in the two cases differ as to their meanings.

Series B VII: (3—1+) "The second experience less intense than the first. In the first, distinct strain in the wrist spreading to hand and fore-arm. The second noted because of lack of intensity" (L). (3—5+) "Prominent thing in the first experience was lively kinaesthesis in the fore-arm. The second experience was more intense. Pressure-complex in the hand gave rise to a conflict as to 'equal' or 'longer'" (L). (3—5=) "In both, which were similar, three complexes stood at focus; pressure from finger, dull pressure from base of thumb, and clear but not filled-out sensations from the arm. The three seemed to have a common background; in the second the background was of less intensity. The three factors were equal in all

respects" (L). (3—2+) "With the first rod, I had sensations in the forefinger from the beginning. These were the most prominent part of the experience throughout. There was an immediate clear perception of length. There was a little change in the sensations from the arm. These were superficial and disconnected from the experience. With the second rod, there were first sensations in the heel of the hand. A very indefinite notion of length. Then sensations from the forefinger and the elbow came in. Sensations from forefinger clearest. Notion of length cleared up" (Z). (3—4—). "With first, sensations of equal prominence in forefinger and heel of hand. Sensations of lower clearness in fore-arm. Fairly definite notion of length. Second rod gave an immediate notion of length, mediated by sensations in the forefinger. Much less clear sensations in heel of hand" (Z). (3—1+) "Sensations from heel of hand and from fore-arm came in. Sensations from fore-arm clearest. Very definite notion of length. Then sensations in the forefinger became practically the only component. Length increased in clearness. With second, sensations in forefinger most marked but vague sensations in heel of hand and in arm. Notion of length very clear and definite" (Z). (3—5=) "First, sensations in heel of hand most marked. Sensations in arm and forefinger less clear and less intense. Not a definite notion of length. Then attention went to the sensations in the forefinger. Sensations in the arm dropped out and sensations in heel and finger became equal. With second rod, first there was verbal-motor 'heavier.' At the same time there came in marked sensations in the forefinger; lesser sensations in heel of hand and in fore-arm. Sensations in finger more intense and deep-seated than in heel of hand. Perception of length came very clearly" (Z).

Center of Mass

Since we took as our criterion for the conditions that determine the formation of the perception of length the ability, again objectively determined, to make judgments of comparative length, we must look for that factor, if any such there be, between which and the judgments we find concomitant variation. Fortunately we find such a one; the distribution of judgments, in every series where the center of mass noticeably varied, gives an X or cross with no inversions of the first order; in every one where the center of mass was constant the curves are flat and irregular. (See Figs. 1, 2, 6, 9, 10, 11, 12, 13b, and 16.)

But if center of mass plays so important a part, as a moment of stimulus, in the arousal of the perception of length, it becomes necessary to inquire more in detail as to its mode of operation. This enquiry should lead us to an understanding of the psychology of the perception, and should contribute toward its psychological analysis. As has been previously stated, a rod or instrument in the hand or a tall hat on the head acts as a simple lever or couple: if, that is, the rod is held out in the horizontal plane, or the hat is worn slightly tilted.

Lotze has pointed out that the tilting is an essential condition of the 'feeling' of enhanced height and consequent greater significance. This means, first of all, that there are two opposing forces or pressures. If we hold a rod in the vertical, there is nothing but the weight; no notion of length arises. It is only as the instrument becomes inclined that the perception of length comes in; weight without an extended center of mass never gives rise to the perception of length; though we do not at all deny that, given a center of mass 'out there,' variations in weight may set off a kind of derived judgment of difference of length.

The absolute intensity of the two impressions can not be the basis for the judgment of length; for we judge a long light rod, which produces relatively weak pressures, as longer than a short heavy bar, which produces much more intense impressions. The answer to the question: What is the experiential foundation for the perception? thus becomes clear; our process of elimination and our introspective analyses lead us to the same result: *the ratio between the 'kick' and the 'pressure.'* In a short rod, the amount of 'kick' relatively to the 'pressure' is small; in long rods, the two pressures more nearly approach each other in intensity; with every change in center of mass there takes place a change in the ratio of 'kick' to 'pressure.' A sensation-complex of two opposed pressures, more or less fused in experience, differing in intensity within certain ratio-limits, is the sensory pattern fundamental for our perception of length as given cutaneously; though sensory or imaginal elements of other kinds may touch-off or carry the meaning. Thus it may be intensity, perceived as weight or taken as bare intensity; it may be the temporal factor; it may be the direction in which the pressures are perceived as acting upon the hand; it may be the strain sensations in the hand; it may be other processes or perceptions than these which touch off the meaning of length or which determine the judgment when we make a comparison. All such secondary cues influence the comparative judgment more than the absolute; they determine differences of length under conditions that would not admit them into the absolute judgment.

A change of ratio of intensity between our two pressures, then, is at the core of the perception of difference of length; a change realized only by varying the center of mass. A variable weight brings no corresponding change in the ratio of the two pressure-intensities; it increases both in the same relative proportion; the ratio remains constant.

Without going into a detailed discussion of the sets of

curves referred to in this section,—they speak for themselves,—we turn to the consideration of some special items of work. We have already found that, with the artificial thumb and forefinger eliminating the 'kick' and the 'pressure' in turn, the perception failed. We found, however, that when we attached the board to the arm in such a way that the band, holding the board to the arm, stimulated a single small area, the sensations from this point combine with those from the forefinger to give a perception of length. This result evidently means that sensations from other and more remote parts of the body, if they are opposed and if they stand intensively within certain ratio-limits to each other, will fuse into a total to give the perception. It is for this reason that the fisherman can feel the pull of the fish on the end of his line as 'out there;' he has the perception of the length of the fishing-rod, even though he hold it over the lower and under the upper parts of his arm. He might just as well hold it over the lower and under the upper parts of his leg; the sensations yielded up by these distant points of contact give the perception as immediately as do the nearer together and more practised parts of the hand. We made no determination of the precision of judgments of length when the sources of sensation are far apart on the body, as compared with the accuracy of discrimination when the parts are near together.

One other modification must be reported. For a short time we told the *O*'s to grip the handles very tightly. With long rods of considerable weight the perception of length was not wholly lacking; the ability to make judgments of comparative length, within the limits employed, was totally nullified. Whether there is or is not a perception of length, with rods of different length, thus depends upon the strength of the grip; if the grip be strong enough, the long rod ceases to be so taken. This can mean nothing more than that the small increments or variations in pressure are too slight to change the ratio in a perceptible degree.

In Table II we give the 'pressure' and 'kick,' their increments, their ratio to each other, and the change of ratio with successive rods, for a few selected series. The series given represent almost all the different combinations of moments employed.

Mention was made in the reports from those series in which weight was the variable of an experience that, while negating the factor of weight, yielded positive testimony to the importance of the two related pressures: it was called the feeling of balance or unbalance. In the following quotations

TABLE II.

Series	Rod	Fore Pr	F.Pr. Incr.	Back Pr	B.Pr. Incr.	Ratio of B.P:F.P.	Ratio Incr.
A I	1	248.23		153.23		1:1.6306	
	2	270.20	21.97	172.20	19.97	1:1.5691	.0615
	3	293.86	23.66	193.86	20.66	1:1.5185	.0506
	4	316.20	23.34	214.30	21.34	1:1.4761	.0424
	5	340.23	24.03	236.23	22.03	1:1.4402	.0359
A II	1	268.91		164.91		1:1.6306	
	2	286.74	17.83	182.74	17.83	1:1.5691	.0615
	3	304.57	17.83	200.57	17.83	1:1.5185	.0506
	4	323.40	17.83	218.40	17.83	1:1.4761	.0424
	5	340.23	17.83	236.23	17.83	1:1.4402	.0359
A III	1	170.06		74.06		1:2.2962	
	2	175.70	5.64	77.70	3.64	1:2.2613	.0349
	3	181.43	5.73	81.43	3.73	1:2.2280	.0333
	4	187.24	5.81	85.24	3.81	1:2.1966	.0314
	5	193.14	5.90	89.14	3.90	1:2.1667	.0299
A IV	1	314.06		218.06		1:1.4402	
	2	320.60	6.54	222.60	4.54	1:1.4402	.0000
	3	327.14	6.54	227.14	4.54	1:1.4402	.0000
	4	333.68	6.54	231.68	4.54	1:1.4402	.0000
	5	340.22	6.54	236.22	4.54	1:1.4402	.0000
B VI	1	787.50		587.50		1:1.3404	
	2	815.00	27.50	615.00	27.50	1:1.3252	.0152
	3	842.50	27.50	642.50	27.50	1:1.3113	.0137
	4	870.00	27.50	670.00	27.50	1:1.2985	.0128
	5	897.50	27.50	697.50	27.50	1:1.2867	.0118
B VII	1	808.80		616.80		1:1.3113	
	2	825.65	16.85	629.65	12.85	1:1.3113	.0000
	3	842.50	16.85	642.50	12.85	1:1.3113	.0000
	4	859.35	16.85	655.35	12.85	1:1.3113	.0000
	5	876.20	16.85	668.20	12.85	1:1.3113	.0000
F IV	1	352.50		252.50		1:1.3960	
	2	375.23	22.73	275.23	22.73	1:1.3633	.0327
	3	400.00	24.77	300.00	24.77	1:1.3333	.0300
	4	427.00	27.00	327.00	27.00	1:1.3058	.0275
	5	456.43	29.43	356.43	29.43	1:1.2806	.0252
F V	1	400.00		286.53		1:1.3960	
	2	400.00	00.00	293.41	6.88	1:1.3633	.0327
	3	400.00	00.00	300.00	6.59	1:1.3333	.0300
	4	400.00	00.00	306.32	6.32	1:1.3058	.0275
	5	400.00	00.00	312.35	6.03	1:1.2806	.0252
F VI	1	418.82		300.00		1:1.3960	
	2	408.99	9.83	300.00	00.00	1:1.3633	.0327
	3	400.00	8.99	300.00	00.00	1:1.3333	.0300
	4	391.74	8.26	300.00	00.00	1:1.3058	.0275
	5	384.18	7.56	300.00	00.00	1:1.2806	.0252

taken from the series in which center of mass varied, we have still further proof of a positive character.

Series A I: (3—4+) "Greater strain on back of hand. Pressure sensations from back of hand greater in second" (F). (4—3—) "Wrist same. Strain factor from fingers to wrist same. Push down on the fingers the same. The thing seems to depend on the pull down from away out there" (F). (1—3+) "Pull in wrist greater and more of 'feeling of going down;' pressure down on thumb and two small fingers. Also pull on back of hand. Pull on back due to end of handle pressing on palm of hand" (F). (3—2+) "The strain-feeling in wrist greater for the second. Also a pull on hand, especially on front finger and back of hand. A sort of stretch" (F). (3—3=) "Pressure alone vivid. Set for pressure. Perceived quantitatively. 'Long' applied to pressure. Notion of rod did not come in. In the second experience, the pressure was the clear part and set off the judgment automatically. Rod context came afterward" (G).

The above report implies, as does many another, a difference between the 'bare length' attitude and the 'length of *rod*' or objective determination.

(3—2=) "Positive judgment set off by hand-complex and slight increase as rod pulls down. This gives meaning that rod is there. Judged on basis of pressure-complexes which were the same in both cases" (G). (3—5+) "Nothing in the first except the perception of pressure and measuring of it. This carried in memory-after-image. 'Longer' came with the second quite automatically with increased pressure. I really judged in terms of heaviness. Length-notion came after" (G). (3—1—) "Resistance less with the second; it came right up. With the first there was more pressure on the back of the hand; the second, even pressure" (N). (3—3=) "I concentrated attention on the shoulder. The shoulder gives criteria for weight. The perception of length seems to come from the pressures in the hand" (N). (3—5+) "With the second, more intense pressure on the fingers and relatively more intense on back part of hand where handle comes against it. This gives length; the length gives pressure at two points" (N). (3—3=) "It is the balance-pattern in the hand that gives notion of extent. It is in this way different from mere weight" (N). (2—3+) "In lifting the first rod there was a lightness of pressure in the hand. The pressure in the hand, with the second, was greater as well as the weight" (Z). "I think in terms of length in relation to compactness or concentration of weight. That is, if the weight is closely compact, it may cause as great a pressure with a short leverage as a more distributed weight on a longer leverage" (Z). (3—4=) "In lifting the former object, I was conscious of an object of considerable length. In lifting the latter rod, I got a notion of weight and pressure in the hand due to leverage. The latter no longer than the former though considerably heavier" (Z). (3—3=) "The clear processes were pressure-sensations from forefinger and base of hand. Judgment was made immediately on basis of comparison of these two sensory complexes. No definite notion of length in either case. Pressure-pattern, that is, the relative distribution of pressure at these two points, was the sole criterion for judgment. They were the same in both cases" (H). (3—5+) "Pressure greater on back of hand. I didn't notice much difference in the forefinger" (H) (3—5+) "Again, pressure at those two points, only, came into the experience.

In lifting the second rod, the pressure seemed to be a little bigger spatially; larger rather than more intense. Gave rise to notion of greater length; greater leverage" (H). (3—4+) "The only mental processes which enter into the judgment at all are pressure sensations from the hand, especially from forefinger and base of hand. In this case, the sensations from these two points, particularly the back, were a little more intense with the second rod. This distribution of pressure gave me the perception of length, although no particular length was assigned to either of the rods" (H).

In the above series weight, as well as center of mass, was a variable component. The following reports were made when center of mass and length were variables.

Series A II: (4—3=) "Quantitative judgment of the first. Verbal-motor, 'great.' With the second there was a tendency to call it lighter. Then the pressures came to clearness and carried the meaning 'equal.' The reaction followed" (G). (3—1—) "Pressure-complex, qualitative and spatial, in hand. There was no notion of rod. In the second, the pressure-complex was clear. Judgment came immediately. Qualitative and spatial pattern and not weight or quantity is significant" (G). (3—3=) "The clear sensations came from the hand. The pull down on the second finger, and the use of the forefinger as guide while it is barely in contact with the handle. Also pressure from the base of hand" (N). (2—3+) "Pull on skin in addition to the greater pressure and pull of the second all gave meaning of greater unbalance" (N). (3—3=) "With first rod, pressure in hand the clearest part of the experience. Length was assigned mainly in terms of pressure in hand. Apparently forefinger acts as fulcrum between the sensations in back part of hand and arm, on the one hand, and length of rod, on the other. Can't differentiate any mark by which I become aware of weight separate and apart from those whereby I assign length: *i. e.*, at the same time that I assign length there is also a definite assignment of weight. When the weight association comes in, the judgment is mediate" (Z). (3—4+) "In first, I was aware of muscular sensations in the arm, then of sensations of pressure in my hand. There was immediate definite length. With the latter rod, there were also sensations in arm and hand. It appeared that the long rod was acting as a lever over the hand. This was assigned length due to the pressure exerted in the hand by the leverage" (Z). (3—5+) "More stretch or pull in wrist. More pull in back of hand with the second" (F). (2—3+) "Little pull in wrist with the first. The pull a little greater in the second. Seemed to be the same weight and the same pressure on the finger in both. Pull in wrist as if the thing reached out longer and gave more pull in wrist" (F).

It may be recalled that the pull in the wrist for F is the sensory experience set up by the back end of the handle pressing up.

(3—1—) "With first, pressure-pattern-complex clear. Second, sensations in forefinger first. Then sensations at base of hand set off judgment 'shorter.' These were less than with the former rod" (H). (3—3=) "Clear thing in first experience was pressure in hand. With second rod pressures in hand very clear and equal to the former experience. Judgment followed immediately" (H).

Series A VI: (3—5+) "Perceived the first as fairly long rod.

Criterion simply intensity of pressure. Pressure fairly intense and a little more so away from body. Second more vivid and alive. Pressure very intense on outer finger. Perceived as 'longer.' Then notion of long dipping rod" (G). (3—1—) "Got notion of length; part of immediate perception. Then sensory experience became clear; live, fluctuating, more intense away from body. Second, perceived immediately as 'shorter.' Perfectly uniform pressure without change" (G). (3—5+) "Greater pressure on fingers and greater pull in wrist with second" (F). (3—2=) "Pressure on fingers and pull in wrist same" (F).

By this time the perception of length came quite immediately, and the *O*'s were often unable to get a vivid sensory experience or a full perceptual realization of length before the meaning itself became clear. There is, moreover, some evidence that the meaning may develop without any process-content at all becoming vivid.

(3—5+) "With first rod an immediate definite notion of length in motor terms and it remained stable throughout. With second, much greater relative pressure and in a flash seemed very much longer with verbal-motor 'longer'" (Z). (3—2=) "The first gave immediately, in motor terms, a fairly definite notion of length. Sensations in hand then became clear. With second, again all at once, upon lifting the rod, I got notion of length in motor terms. Verbal-motor 'equal.' The pressure-sensations again became clear and were equal to the former. The critical comparison or criticism of the automatically rendered judgment based on pressures in hand" (Z). (3—4+) "Judgment made on quantitative basis. Second, pressure at base of hand more intense relatively to pressure on forefinger, and the entire pressure-pattern more intense than the first. First, like well balanced rod; second good deal of pull down to it" (H). (3—1—) "In first, pressure moderate, evenly distributed. A well balanced rod. An immediate perception of length. In second, pressure was much less *in toto* and pressure at base of hand very slight. A little rod tipping up. 'Shorter' came as a part of the perception of length" (H). (3—5+) "In all these cases, the perception of length comes immediately. Sensory basis becomes clear afterward. Pressure in first that of a well balanced rod; liveness gave meaning of moderate length. With second, pressure more intense; strong pull down. Tendency after judgment to call the second heavier. Intensity o pressure at base of hand meant 'longness'" (H).

The series in which we used the heavy rods gave us reports of the same general bearing.

Series B I: (3—5+) "Pressure more on three fingers and muscular sensation in fingers. In the second, intensity of pressure on palm of hand and base of thumb. More muscular sensation in the whole hand, in the second" (S). (3—5+) "Pressure and muscular sensations greater in second. Also deep pressure-sensation in thumb and greater tension in second" (S). (3—4+) "All in hand. Pressure a little in second. Muscular complex in hand. Hand adjusts itself a little differently to different lengths" (S). (3—2—) "Muscular tension in fingers. Pressure on thumb. Second, pressure on thumb and muscular tension in fingers. Pressure less in last experience" (S). (3—1—) "Second came up easier, and it didn't drag or pull the forefinger down as a long one would" (S).

The strain-sensations set up in the hand as the organism tends to resist the two opposing pressures and to hold the rod in the horizontal position, enter now as a new secondary factor for carrying the meaning of length.

(3—2—) "The meaning, length, is placed upon the differential pressure-sensations in the hand; *i. e.*, the greater or less pressure on the forefinger *versus* that on the heel of the thumb carries meaning of length somehow, not visually, projected beyond the hand. The hand has come to mean fulcrum" (L). (3—5+). "In the first rod the pressure-complex was clear. Deep pressure was especially prominent. In the second case, the strain-sensations in the arm seemed coordinated with the pressure-sensations in the ball of the thumb" (L). (3—5+) "The handle, in this experience, was grasped less tightly, therefore the differential pressure-complex was more definitely analyzed: the pressure-complex of thumb and finger from that of the palm of hand. Torque was localized in hand. How torque functioned in connection with the pressure-complexes, I can not say. This torque is all in the hand" (L). (3—2—) "Experience centered about sensations in the hand. In the first, a clear differentiation. Light pressure and contact from thumb clear. Also sudden and intense deep pressure from palm of hand. The second experience was the same in the main, only less intense, especially on the thumb. Judgment immediate" (L). (3—1—) "Experience of the usual sort in which the judgment followed immediately. Difference that demanded attention was localized in hand and was of temporal nature. Complex of pressure and kinaesthetic sensations whose course was slower in the first experience than in the second. As to region of hand, contrast between inner side of hand just behind forefinger, and the base of the thumb" (L). (3—4+) "First rod gave clear sensations in forefinger and less clear on back part of hand. Experience in lifting second, sensations of forefinger more intense. Also more extensive. Immediately judged it as 'longer'" (Z).

It often happened, as in this case, that *O* noted the difference of intensity at a single point, say the forefinger; a fact which might seem to contradict our general conclusion. The contradiction would be real, however, only if we were in face of something other than distribution of attention; if *O* did not in some way take account of the relative as over against the absolute variation.

(3—1—) "With the first rod, I experienced, first, fairly clear sensations in the hand, especially on forefinger and at base of thumb. With the second rod, I had sensations in hand, wrist and fore-arm. Attention not demanded at any one point" (Z). (3—4+) "I first noticed, in the first experience, sensations in the index-finger which were very intense and pressed the forefinger down tensely against the wrist. There followed, soon, sensations in ball of thumb. Second rod, somewhat similar experience for the first moment. Pressure on forefinger following, down to wrist, and lesser sensations at base of hand. These accompanied by slightly greater strain-sensations through the hand." (Z). (3—1—) "Dull pressure-sensations at base of thumb especially clear in the two experiences. Much less intense with a somewhat 'bright' quality in the second" (H). (3—3=) "The pressure from the forefinger and base of hand the same in the two experiences. The

clear observation of the sensory complex in the second followed the judgment, which seemed a part of the perception" (H). (3—2—) "The pressure on the forefinger was, in every observable respect, the same for the two experiences. The pressure at the base of the thumb was less intense in the second case. Also it had the meaning of 'liveness' upon it, due to a 'bright' quality and a temporal fluctuation" (H).

Since section B in a sense duplicated section A, only a part of the *O*'s were required to perform both series. It is for this reason that we have introspections from two or three only.

Series B II: (3—4—) "Strain in skin, forefinger and hand. The second more intense, with increased pressure on the thumb" (S). (3—1—) "Pressure in palm of hand, in first. A little strain in first finger. Less strain and less pressure in the second" (S). (3—4=) "In first strain in fingers and down through palm of hand all as one sensation. Second, same in distribution and intensity" (S). (3—1—) "Strain-sensations in finger and palm of hand. Pressure-pattern at base of thumb. Second, lack of strain-sensation in hand, less intense in fingers" (S). (3—5+) "Pressure on forefinger and at base of thumb the clear processes. Less clear muscular and strain-sensations through hand. Marked increase in intensity at base of thumb; less marked increase on forefinger. The second pressure more extensive, giving rise to notion of its pulling down more" (H). (3—2—) "The two experiences were qualitatively the same. The second was a little less intense at the base of thumb. The second also less extensive. Strain-sensations through the hand the same" (H).

Series B VI: (3—4+) "Most prominent distinction between the two, and which seemed to carry meaning 'longer,' was the temporal sequence in sensations at base of thumb. In the second they rose more slowly. Temporal course in first more rapid. This may have been confused with amount of change passed through. Greater range in second; *i. e.*, the relative amount of increase of intensity was greater with the second for this particular complex" (L). (3—5=) "The two experiences as usual, except for a peculiarity in second. Began by intense pressure on thumb, and this brought at once the meaning 'longer.' But pressure decreased and then increased on forefinger. This negated the earlier intense pressure against base of thumb" (L). (3—1—) "The sensation-area from forefinger constant and in background of consciousness in both. In first, the pressure at base of thumb showed a transitional change and rose to high intensity as a block of experience lifted out. In the second, the whole thing seemed a flat pattern with only a vaguely traced boundary between pressure on forefinger and thumb" (L).

All these reports from the introspective data corroborate the findings from the psychophysical series as presented in graphical form.

Synthetic Series

The best test of any hypothesis regarding the analysis of a perception is that of reconstruction under the observed conditions. If our assumptions are well founded, then the perception of an extent beyond the hand should be aroused by any two pressures, so long as these are kept between certain limits of

ratio of intensity, of spatial relationship, and of direction. Taking these facts as our point of departure, we set out to produce the perception synthetically.

The matter of thus setting up the length-experience with no length present reduced itself finally to the problem of devising a means by which the two opposing pressures could be applied to the hand (since this was the part of the body with which we had worked) in a way that should be under perfect control. We found that when we took two short pieces of wood like the handles, and pressed the one up against the base of the thumb and the other down upon the forefinger, our *O*'s gave immediate judgments of length: in fact they were unaware, according to their own report, of the substitution of the two short pieces for the one long rod. Further preliminary trials proved that the length, as perceived, could be made 'longer' or 'shorter' by alternately increasing or decreasing the 'kick' and decreasing or increasing the 'pressure;' and that the perception broke down if *E* increased either 'kick' or 'pressure' beyond certain limits, when *O* perceived the two pressures separately as two unrelated experiences. So far, however, our pressures were not subject to rigid control.

The problem demanded two physical pressures, which were to be resisted independently, to operate in opposite directions, to be stable in duration, definite in amount, and independently variable. The first apparatus consisted of a handle, with an oval hole cut through lengthwise, slipped over a small rigidly supported rod set in the horizontal plane. A short pin, set into small holes bored through handle and rod at right angles to their principal dimension and at close intervals, served as a pivot, so that the handle had a considerable latitude for movement up and down about the pivotal point. By altering the point of pivoting, the length of the two lever arms changed inversely, while at the same time the down-pressure of the fore end and the up-kick of the back changed in an inverse ratio. Since the effective physical pressure is a function of the length of lever arm and of the weight or force applied, these pressures could be determined and varied by assignable amounts; the weight, hung at the front end of the handle, tended to actuate rotation of the handle in the hand of *O*, who gently resisted the movement. The device was a success so far as the evoking of the perception of length was concerned. It was, however, ill-adapted to the evoking of judgments of comparative length, since *O* might, by using the pivot as a fulcrum, resist all force of the weight at either the fore or the back point of contact.

Next a split handle was arranged, with the top half pivoted in front and moving upward through a small arc at the back, and the bottom half pivoted behind and moving downward; the movement was produced by weights attached to cords which ran over pulleys. The two pressures could be varied at will, and must be resisted independently. The perception of length did not invariably result, for the *O*'s were immediately aware of the division in the handle, and might allow the one half to move through a relatively large arc while they held the other from moving at all.

This apparatus was given up as a failure, and in its place we made a handle with a smooth horizontal hinge at the middle; the back end being pushed up and the fore end pulled down by easily variable and definite amounts. *O* took the handle loosely and the weights released caused the handle to dip down in front and up behind, just as a rod would behave. So long as the

handle did not 'break' at the hinge, the perception of length was definite and controllable; but a 'break' destroyed the perception, since the two pressures were no longer 'in line.'

Finally we hit upon a simple and satisfactory arrangement; a handle suspended on a cord attached at the front end. Fastened to this handle were two weights; the one, fixed to the lower side at the front end, pulled down; the other, fastened to the back and by means of a cord run over a pair of pulleys, pulled up. There remained one disturbing factor; the handle was unstable; it seemed unattached. By inserting a light rod into the handle, the possible length-effect of which we overcame by a weight on a heavy wire set into the rod and bent back under the handle in such a way that the center of mass was at the point of the forefinger, we secured better results. But the rod was presently dispensed with. We suspended the front weight, which had been rigidly attached to the front of the handle by a short wooden piece, on a cord and so eliminated the rotation-effect due to the swing of the weight. After lengthening this cord which held the front weight, so that its pendular period was about the same as that of a normal rod which would give the same 'kick-pressure' ratio as the two pressures from the handle, we took out the rod; a bare handle remained.

The front weight was 250 gr.; the back pull was 140 gr., increased and decreased by 20 gr. increments. To be sure, the two pressures were not wholly independent of each other; if the pull or 'kick' did not come directly under the point of contact between the hand and handle, any increase or decrease of the pull would be reflected by a change in the amount of pressure necessary to counteract the weight in front. We can not be absolutely sure that with *O*'s whose hands differed considerably in size we met this objection. But even if the front pressure varied somewhat from the amount of the weight, it could have done nothing more than change the ratio of 'kick' to 'pressure;' it would have been a constant error for any one *O*. According as it increased or decreased the absolute intensity, it would have made the small changes less or more easily perceived.

Two features connected with the lifting of the handle called for special practice; *O* must lift the handle in such a way that the two pressures came on simultaneously, and must oppose the back pull sufficiently to prevent that end of the handle from moving upward.

Fig. 18 gives the graphical representation of the results; the abscissae correspond with the five settings of the back pull which alone varied. Here are comparative length-judgments made under the same instructions as the judgments with actual rods. The results speak for themselves. There can be no doubt of the actuality of the perception, or of the effectiveness of small changes in the intensity-ratio of 'kick' to 'pressure' for altering it: the perception of length can be synthetically produced, and is so accurately conditioned that very small changes in a single pressure bring about corresponding changes in the meaning of length. A comparison of the results of series F IV with those of F V and VI (Figs. 24, 25 and 26) shows that variation of a single pressure is less effective for determining the perception of length than is a variation in both

pressures, changes in center of mass being the same; and a comparison of F V and F VI shows that, when the fore pressure is constant (see Table II), the back pressure variable, and the center of mass still the same for corresponding rods in the two sets, we get higher percentages of the 'longer' and 'shorter' judgments than when the reverse is true. We had not the results of section F before us when we performed the synthetic work.

The reports from the synthetic series show that the same two pressures demand attention under the synthetic situation as in the case of true rods.

Synthetic Series: (3—5+) "Strain-sensations run down wrist and are fused with the hand-sensations. The pressures come on forefinger and at base of thumb. These latter are objective" (Ca). (3—5+) "Pressure on index-finger most intense. More intense in second. Pressure on other fingers less intense. Pressure on thumb was uniform" (Ca). (5—3—) "In first, pressure on forefinger. Irregularity in change of intensity of pressure. In second, pressure was constant in intensity. Pressure on thumb and ball of thumb. Constant sensations in other fingers" (Co). (3—5+). "Pressure-sensations on forefinger and thumb, more extensive on thumb with second. Pressure on forefinger slightly preceded that on thumb. The two must be combined to give the total experience" (Co). (1—3+) "Pressure on forefinger and in palm of hand, at base of thumb and at tip of little finger. More intense pressure on palm of hand and little finger in second" (Co). (3—1—) "Very intense pressure at base of first finger and intense pressure at base of thumb. The pressure in the second case less intense. They came in simultaneously" (S). (3—5+) "Pressure deep at base of first finger and pressure on thumb, about the middle. Clearer and more intense on finger. Area shifted outward a little. In second, deep pressure on finger, more intense and changed in area, more spread out than first. Pressure on thumb less clear, more intense than in first" (S). (3—2—) "Dull pressure on forefinger and at base of thumb in both. Vague sensations in lower part of arm. Less intense in second case. The pressure at base of thumb less in second, gave meaning of slightly less unbalance" (H). (3—4+) "With the second, the pressure at the base of the thumb was more nearly equal in intensity to that on the forefinger, than in the first. No qualitative difference noted" (H). (5—3—) "Marked pressure sensations on base of thumb, in the first. Less clear sensations on forefinger. With second, the sensations on forefinger clearest. Those at base of thumb less intense" (Z). (3—1—) "First experience carefully noted. No definite idea of length. Judgment part of second. When analyzed, it was less intense at base of thumb and less strain in hand" (L).

V. Secondary Factors

Visual Imagery

All *O*'s reported visual images, more especially during the early series; Z reported images of this kind, more or less, throughout the whole of the two years. In all cases, however, the image was an immediately accruing process which derived its meaning from the

sensory complex in the hand. Frequently it was instable, fluctuating with changes of eye-convergence. Frequently *O* volunteered the statement that it came after the judgment and served merely a confirmatory purpose; it was his justification. Furthermore, the coming in of visual images bears a close relation to the giving, under the comparative determination, of judgments of absolute length and of reflective judgments. In fact, visual imagery was never more than a secondary factor in the perception; it never came alone or even first in temporal sequence; it was far more likely than not to occur in the case of the reflective judgment or to follow the judgment; and it never appeared when the judgment was a part of the perception.

The following reports show the symbolic character of the images, the temporal order of their occurrence, and the relation they hold to the notion of length and to the comparative judgment.

"Image and eye-movement. No notion of length. Just rod lying out there going into third dimension. This image carried right over into the second experience. But it had nothing to do with the judgment. Judgment made on basis of pressure solely" (G). "As soon as judgment was rendered, a visual image meaning length came in fragmentarily. Also eye movement. All as I am reporting and after." (G). "After judgment, visual gray spot for a second. Some eye-movements entered into spacing. Confirming" (G). "Process, gray thing, developed by eye-movement. When sighting where I thought the thing would be, image came in" (G). "Visual image. Eye-strain determined distance" (G). "Accompanying judgment was a composite visual schema of two lines, one longer than the other, in a vague way connected with the hand. Visual image not at all connected with the judgment" (L). "Slight visual imagery, very schematic. Not at all a reproduction. Visual image supplementary" (L). "Visualization not important. Weak, hazy, no clear outlines, insignificant" (F). "Visualization. Second, lighter gray. Spatially related, eight inches between them. Darker one farther out. Second lighter, nearer in" (F). "Visual image of a flashing up of the end of the rod gives a rough outline of length" (N). "Scrappy bit of visual imagery came in after the judgment. It meant the end of the rod" (H). "When I had lifted the last rod, I had a visual image of hand lifting two rods in succession. At first the two were not definitely outlined as to length. In a moment the image of the first rod appeared as extended about two inches beyond that of the second. This image was vague except at the hand" (Z). "In this experience I had very clear visual images. Clearest part of image connected with lifting the last rod was at distant end of rod. End of rod was much heavier than first. It seemed as if it were bent down at the end" (Z). "As soon as I lifted the first rod there appeared a definite extent in space from me. The nature of this I do not know, for almost simultaneously I had a visual image of a rod the distant end of which was the clearest part. I rather think there was something there before the visual image appeared" (Z).

The introspections show a relation, more or less definite, between the quality of the image and the perceived weight or length. For F the darker gray always stood for the rod producing the more intense pressure; Z distinguished the dark, solid-grained oak from the soft, light colored poplar; and H occasionally correlated 'light and short' with bright tint. Always, however the perception of weight or of length was primary. There is also a relation between the tendencies to

visualize and to objectify the stimulus-object. So long as *O* perceived length as bare length, unrelated to an object, the visual image did not enter; when he perceived length as the length of a particular object, the objectification often took place in visual terms.

Muscular, Strain and Articular Sensations

James [12] says of projected movements that all the intervening space must be represented in consciousness; that when the projection is to the end of a walking stick, the extent of this stick must be represented in visual, tactual or kinaesthetic terms. James is concerned with projection and not with bare length; but in the case of judgments of absolute length, with objectification of the experience, we have a very similar condition. We found muscular, tendinous and articular experiences reported by our *O*'s, for the most part as sensory and not as imaginal. Z reported them more consistently and for a longer time than any other *O*; Ca reported them during the weight-series. These sensations compelled attention by their intensive changes. The arm seemed to be the seat of the only variable experience; and hence to afford the psychologically adequate basis for making differential judgments. It may be that Z had recourse to visual and motor imagery and to the reflective judgment more than any other *O* because for him the successive perceptions were differently determined. That is, the clear and, for him, the significant part of the first experience might be arm sensations, for the second, the hand-complex. Then the visual images evoked by the two would come together as the common element for making the comparison. One of his reports will make this state of things clear.

(3—1—) "With the first rod, I had, first, sensations in the hand. I did not get a definite notion of length from the sensations in the hand. Then there were very marked sensations from the arm. Meaning of length took on more definiteness. These sensations from the fore-arm remained clear throughout the experience. With the second rod, I had sensations in hand and also given at the same time the notion of 'lighter.' Visual image of picture given by Ebbinghaus in connection with a discussion of looking at two grays. Notion of 'lighter' pretty well marked. Verbal-motor: 'Is it shorter?' Notion of length seemed to vacillate. Again sensations in arm came in, and seemed to help in the attainment of definiteness of length. Visual image of the two rods of about the same length. Heavier one dark and of heavy wood; the light one, a poplar rod. Judgment based on the visual images" (Z).

Experiences of such elaborate pattern were rarely reported; the imagery was more often of the motor or kinaesthetic than of the visual type. But almost without fail, when the successive perceptions had a different sensory determination, *i. e.*, when different sensory processes were clear, some type of imagery served to represent at least one member of the two experiences which came in for reflective comparison. It is significant that the other *O*'s seldom reported kinaesthetic sensations, but they almost never gave the dual determination of the two experiences, and Z rarely reported kinaesthesis in both experiences. Though, when sensations from the arm were clear, the perception of weight almost always intervened, it has yet to be shown that sensation from this source can function in any other than a

[12] W. James. *The Principles of Psychology*, 1890, II, 196.

vicarious manner; it is more probable that the strain and muscular sensations from the hand and, to a lesser degree, those from the wrist are the psychological correlate of the meaning of length; the part they play would still be secondary and not primary.

When we used the wooden 'spoon' to take up the 'kick' of the rod, L, reported a 'thinness' or 'emptiness' of the experience, which he attributed to the elimination of the hand and wrist kinaesthesis; this kinaesthesis had served as a background for the total experience. With the heavier rods, S repeatedly reports muscular and strain sensations from the hand aroused by the leverage of the rod held in the horizontal position. All these things are very different, however, from the kinaesthetic image an *O* would derive from having laid the rod along the body and leg, or from having run the hand along its extent.

The Temporal Factor

There were at least three, more or less distinct, ways in which the temporal factor entered into the complex of experience to affect the judgments both of length and of comparative length. These were the quickness with which a short or light rod came up, especially when lifted after a longer or a heavier rod; the time, independent of the lifting, within which the pressure-pattern formed in the hand; and the degree of steadiness exhibited by the pressure, as 'dead' and unchanging or as 'live' and fluctuating. When the first of these modes was an element within the complex of experience, the judgment 'shorter' or a tendency toward that judgment followed, if the rod came up quickly, and contrariwise if it came up slowly. The former happened much more frequently than the latter. If *O* postponed judgment until after the rod became stable, he frequently reversed his judgment, if it was a matter of difference of weight.

Some of the pressure-patterns, in accordance with the second principle given above, formed immediately when the lifted rod became an effective stimulus, and remained unchanged throughout the experience; others showed a well marked course of development in intensity, extent, or (less commonly) quality. In general, an increase of intensity or extent carried with it the meaning 'longer,' as did a shift of quality in the direction of ache or strain.

The 'liveness' of the pressure, required by the third principle, differed from the change mentioned under the second heading in that it was not a growth in a constant direction. As a rule, an unusually 'dead' static pressure carried the meaning 'long;' the 'live' fluctuating pressure, the meaning 'short.'

These three aspects of the temporal factor hardly ever occurred in combination in a single experience. Illustrative reports follow.

"As pressure creeps it becomes more intense on index finger and less intense on little finger. Second pressure intermittent, up and down, on forefinger" (Ca). "Second gave very slow intermittent pressures" (Ca). "Sensations on forefinger varied in intensity, also in a temporal way" (Co). "The difference is largely a temporal one in the forefinger. In the first, there was fluctuation; in second, constant. Especially fluctuations in muscular and tendinous sensations of the forefinger" (Co). "Resistance less. Came right up" (N). "Longer ones more springy; shorter ones come right up quick without any springiness. Greater pressure on the base of the hand with the longer rods is there immediately upon picking them up.

As soon as they are raised the distribution of pressure equalizes and remains constant" (N). "Second pressure at base of hand variable in time. It had a 'lively' quality, go and come" (H). "A sudden intensity means weight; length comes slower" (F). "Strain-sensation in fingers and palm of hand. Also deep pressure, mixed together, and it grew in intensity relatively slowly. This gave the meaning 'longer'" (S). "Second, slower strain-sensation in fingers and on thumb. Rose to maximal intensity more slowly than first and carried meaning 'longer'" (S). "The temporal course of processes seemed to be the most important. Torque and temporal bound up very closely" (L). "Second experience, whole experience less in intensity and showed none of the temporal course. Second, 'shorter'" (L). "Temporal sequence in this case clearly observed. Slower in second case; *i. e.*, the one to which 'longer' attached. The thumb-complex rose slowly to full intensity" (L). "Arm went up so much faster than it ought. Too much energy. It was surprisingly short" (C). "First, pressure 'alive,' 'gay.' Perception bound up with greater intensity that meant 'dead' weight and not fluctuation" (G). "Second, got more intense and spread out as if rod was bending down" (G). "I thought both long because of temporal thing, gradual increase in intensity of pressure as they were picked up. The proportional relation of amount of intensity and the rate of increase were noted" (G). "There seemed to be something like a resistance, a give or vibration or something, that gave the notion of length as distinct from the dead pressure from heaviness" (G). "Second seemed to go up. Sudden change in pressure, lightening of pressure, gave notion of 'short'" (G). "Fluctuation, more and then less intense, gave perception of fairly long dipping rod" (G).

Pressure Gradient

There are, no doubt, other secondary criteria upon which the perception of length, in the sense of this study, may be based. We have found, *e. g.*, that if a ring is made fast to the skin by means of a gummed tape, and a pull is exerted on this ring in a direction away from the body while at the same time a pressure of slightly different intensity operates upon a nearby spot on the skin, the perception of length follows: the extent is in the direction of the 'pressure' if this is more intense than the 'pull;' in the opposite direction, if it is less. Gradients in either direction are thus adequate to evoke the meaning of length, provided always that they are opposite and slightly different in amount. Moreover, the steepness and direction of the gradient, in the hand at least, are factors that on occasion play a part in determining the meaning of length.

The Absolute Length-Judgment

As in the case of lifted weights, the judgment of absolute length appeared; all *O*'s reported it at times. The judgment usually occurred with rods of the one extreme or the other; practically the second one was judged before it was lifted. The general instructions failed to the extent that a special disposition or attitude might develop which prevented *O* from doing his assigned task. Suppose, *e. g.*, that *O* judges the first rod as 'very long' in absolute terms; then he will expect, pre-judge, the next as 'shorter:' the consequent 'set' for the next rod is 'short;' the characteristic intensive, temporal and other differences are anticipated.

This absolute judgment is, however, of little concern to us in our analysis of the non-visual perception of length, since there is no indication that its determination is in any way different from that of the perception of bare length or of the perception of difference of length. We note only that the visual image, more frequently than any other process, carries the meaning of absolute length. Visual image, absolute length and the reflective judgment are bound up together. The two absolute meanings, visually determined, must be brought into a logical relationship before a comparison is possible. The situation is somewhat different in the case of judging the length of a single rod. Illustrative reports are:

"Visualized two points in space. First, far out; second, near. Two absolute judgments. Relative judgment an inference" (F). "I got the meaning of absolute length with the first. This judgment of absolute 'shortness' for the first gave me a 'set' for 'longer.' I expected the second to be longer. I can't say whether that imposed judgment or not" (H). "Absolute judgment of first as 'long.' Set to expect 'shorter'" (G). "Absolute judgment of first 'long.' Set off by pressure-pattern" (G).

VI. On the Computation of a Limen

The method of Constant Stimulus Differences may and usually does give a *DL*. Still following Urban we sought to use our data for this purpose. We computed the *h'* and *L'* for the results of the several *O*'s in the early series; but there we were halted. What is the unit of stimulus in terms of which a *DL* might be expressed? We had three sets of variables, all of which varied together in direct proportion. Which should be used in correlation with the psychological experience? At first we thought that the *DL* should be expressed in terms of weight; for applied weight is the mechanical stimulus which produces sensations of pressure, and resisted weight, muscular and strain sensations. But the results shown in Fig. 2, as well as those of the other series in which the weights were the same for all rods, prove that the *DL* cannot be found from the objective differences in the weight of the rods. So the graphs given in Figs. 4 and 17 show flat and irregular distributions; the 'kick' and 'pressure' were not equal or constant, but their ratio did not change. Weight is out of the question.

Before the work was far advanced we had decided that length, in and of itself, could in no way affect differentially the arousal of sensory experience; hence we could not correlate length with judgments of length. The results (Figs. 5 and 15) prove the justice of this conclusion. Nor are weight and length combined as co-variables adequate so to determine the perception that accurate comparative judgments are possible (see Figs. 4 and 14). Judgments of length were possible,

since the adequate stimulus for the perception of length was present, but they were undifferentiated. Length, too, is out of the question.

Can we, then, state a *DL* in terms of center of mass? We have already remarked that the intensity-ratio of 'kick' to 'pressure' varies with every change in center of mass; and we have said that it is precisely this ratio that determines, psychophysically, the non-visual perception of length. The distance, in absolute terms, from center of mass to fulcrum-point at the forefinger varies directly with the intensity-ratio of 'kick' to 'pressure,' and the relation of this ratio to another of the same kind in two successive experiences determines the perception of comparative length. Have we here a moment in terms of which we may express a correlation between experience and stimulus-object?

In the first place, we note that changes in this intensity-ratio are effective for modifying the perception only within certain limits, the lower of which we have fairly established. The *O*'s failed to perceive length when the intensity of the 'pressure' became twice that of the 'kick' (series A III, B IIIa and F I). The upper limit we did not determine. In the second place, a number of other secondary factors enter in to influence the perception of length. We have already discussed at length the relation of absolute intensity, as a correlate of weight, to the perception. In addition there are the elements of time, the character of the pressure-gradient, and the general sensory background from the muscles, joints and tendons of hand, wrist and arm, all of which not only touch off the meaning of relative length but may actually modify the perception.

The results from the 'spoon'-series prove, nevertheless, that there can be no perception of length without the two pressures. Here the perception appeared only if there were movement of the rod; as the rod swayed or vibrated there was generated a duality of impression within the limited area of the single contact-point. There is one instance in which the two sensations seem at first sight to come in succession rather than simultaneously: the case of the rod balanced on the end of the finger. As the rod begins to tip over, there comes the knowledge that it is an extended object and not a compact mass. The pressures are, however, not really successive; pressure becomes intensified in one part and weakened in the opposite part. If the end of the rod be pressed obliquely against the skin there is no perception of length. It is only as movement begins, causing the inverse change of intensity in opposite parts of the stimulated area, that we perceive length.

Thus the primacy of the two related pressures forces itself upon us once more.

We thus arrive at the conclusion that the primary item of experience, the underlying and essential condition for the perception, is a ratio between two opposing pressures held together by certain directional and temporal relations. But *for this ratio there is no directly corresponding element or moment in the stimulus-object.* Center of mass, it is true, bears a constant direct relation to this ratio; only, if we have recourse to it, the physical terms in which we express the limen will be two steps removed from the essential item of direct experience. The primary psychological condition of the perception transcends the stimulus-object.

VII. On the Application of Weber's Law

As regards Weber's law, there is a characteristic of our curves of distribution which may have an indicative value. In a majority of cases, when we consider those series only which gave X's, the curves for the judgment 'longer' do not rise as high as the curves for 'shorter,' and most of the inversions and plateaus lie on the upper side of the standard. Our steps, whatever their nature, represent (except in section F) an arithmetical and not a geometrical progression. Stimuli which vary in this way should, under a strict working of Weber's law, give psychophysical results such as we obtained. Our interpretation of the perception of length as based upon a ratio of intensities is thus, so far, confirmed.

Conclusions

Our analysis shows that the non-visual perception of the length of a lifted rod is based primarily upon the experience of two opposed pressures whose intensive ratio falls within certain limits.

There are at least four factors which join with the two opposed pressures, singly or in combination, to complete the perception. These are absolute intensity of the impressions, their temporal course, the pressure gradient formed, and the muscle and strain sensations from hand and arm.

Visual and kinaesthetic images serve to establish the length in absolute terms. They may serve the end of comparison, but then imply a reflective judgment.

The perception is given as accurately in the passive as in the active state.

The psychological substrate of the meaning of length has no direct physical correlate in the stimulus-object.

The perception can be synthetically produced by the application, under certain conditions, of two opposed pressures.

A NEW FORM OF STIMULI FOR LIFTED WEIGHT EXPERIMENTS

By SAMUEL W. FERNBERGER, Clark University.

The questions regarding the form and material of the stimuli to be used in lifted weight experiments have been variously answered by different experiments. Fechner [1] placed his weights in holders which were grasped and lifted with the palm of the hand upward. Galton [2] used simple shotgun shells loaded to the proper intensity with successive layers of shot, cotton wool and wads. These weights were grasped by the subject "between his finger and thumb, the finger pressing against the top, the thumb against the bottom of the cartridge." There is a set of brass weights loaded with paraffin made in this form by the Cambridge Scientific Instrument Company, Ltd. Sanford [3] suggested the use of envelopes weighted with sheet lead, to be lifted vertically suspended between the thumb and forefinger. Presumably weights of this type were intended only for use in an elementary laboratory course. Fullerton and Cattell [4] employed flat wooden boxes weighted with shot and cotton, which were lightly grasped on the side with the thumb and fingers. Urban [5] used weights of the same general form made of hollow brass cylinders loaded with shot and paraffin, which were also lightly grasped on the side between the thumb and fingers. Inasmuch as paraffin is not entirely anhygroscopic, solder was substituted in a second set of weights exactly similar in form and size to those used by Urban.[6] A set of weights made of hard rubber and weighted with shot, advertised in 1894 by the Garden City Model Works, is

[1] G. T. Fechner. *Elemente der Psychophysik.* 2nd Ed., 1889, I., 97f.
[2] F. Galton. *Inquiries into Human Faculty and Its Development.* N. Y., 1883, 34ff, 370ff.
[3] E. C. Sanford. *A Course in Experimental Psychology.* Boston, 1898, 413.
[4] G. S. Fullerton & J. McK. Cattell. *On the Perception of Small Differences.* Phila., 1892, 118f.
[5] F. M. Urban. *The Application of Statistical Methods to the Problems of Psychophysics,* Phila., 1908 1ff.
[6] S. W. Fernberger. On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli. *Psychol. Rev.,* XXI., 1914, 337f.

mentioned by Titchener.[7] They consisted of 9 cylinders, 1½ inches in diameter and 4 inches high.

It seems curious that experimenters have overlooked the obvious fact that the size and form of the stimuli employed in this sort of experimentation may have an effect upon the measures of sensitivity. Experimenters have directly compared the results for lifting weights by the Fechner handles with the results for lifting by grasping the weight itself. It seems obvious that the manner of lifting a weight must affect the results. The processes underlying the formation of judgments in lifted weight experiments are exceedingly complex and involve pressure, muscular, articular and tendinous sensations with widely different points of origin,[8] which might be expected to differ with the various forms of weights and the manner of lifting them. In the face of ignorance the safe course is to use a single standard form. Weights of the size and form suggested by Urban have had a large and satisfactory usage in the hands of a number of recent investigators. They are comfortably grasped between the thumb and forefingers.

The materials of which the weights are composed also require standardization. In the first place the physical weight must be kept constant within the limits of error of the experiment. Urban[9] found that the wooden weights of Fullerton and Cattell varied, with differences in atmospheric temperature and moisture, sometimes more than 15 mg. The brass weights constructed by Urban and the writer overcome this difficulty. Such a set, loaded with solder, in use in the Clark Psychological Laboratory for the past eight years, has not shown for any single weight as much as 10 mg. variation.

Ide[10] has recently found that warm or cold stimuli are overestimated when compared with stimuli which do not arouse temperature sensations. Ide compared cold stimuli of 46° F and warm stimuli of 147° F. with neutral stimuli of 70° F. The resulting variations in the points of subjective equality for the method of constant stimuli are considerable; in some cases the difference is more than three grams.

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[7] E. B. Titchener. *Experimental Psychology.* N. Y., 1905, II., Part 2, 265.

[8] cf. S. W. Fernberger. An Introspective Analysis of the Process of Comparing. *Psychol. Mono.*, XXVI (Whole No. 117), 1919.

[9] Urban. *op. cit.*, 2f.

[10] A. L. Ide. *The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments.* Univ. of Pennsylvania Thesis, Phila., 1919, Pp. 25

Ide has investigated this problem systematically but the general observation appeared in the literature many years ago. In 1846, E. H. Weber [11] observed that a cold coin at —4° to 7° C. placed on the forehead seemed as heavy as two superinposed coins of the same size but of a temperature of 37° to 38° C. Some years later Szabadföldi [12] repeated this experiment but extended the range of warm temperatures beyond that employed by Weber. Szabadföldi found that both very cold and very warm stimuli are overestimated when compared with neutral stimuli which do not arouse temperature sensations. These results are entirely in accord with Ide's findings although in one case the experimenters were dealing with passive pressure and in the other case with active lifting.

In some recent experiments performed by the writer during the winter, the subjects sometimes complained that the brass weights felt cold. Metal readily gives rise to thermal sensations because of its high thermal conductivity. For this reason we have constructed a set of weights from hard rubber which has a low coefficient of conductivity, and which further recommends itself as anhygroscopic, durable and easily worked.

The coefficient of thermal conductivity (calories per cm. per sec. per degree) for yellow brass is about 0.20 and for vulcanite about 0.002.[13] In terms of stimulus and of sensation, if thermal conductivity were the only factor, this difference would mean that the range of temperatures which do not arouse temperature sensations would be 100 times as great for hard rubber as for brass.

Hard rubber seems to have been little used for lifted weights. We noted above one set of rubber weights. The Fechner weight holders listed by Spindler and Hoyer of Göttingen in their catalogue of 1910 have hard rubber handles. But neither of these forms of weights have been used in important experimental work. On the other hand, it has been almost universally recognized in aesthesiometric work that metal points arouse temperature as well as pressure sensations

[11] E. H. Weber. *Der Tastsinn und das Gemeingefühl.* 1846 and also Tastsinn in Wagner's *Handwörterbuch d. Physiol.*, Braunschweig, 1846, III., Part 2.

[12] M. Szabadföldi. Zur Physiologie des Tastsinns. *Unters. z. Naturlehre d. Menschen.* IX., 1865. For an account of these experiments *cf.* O. Funke. Der Tastsinn und die Gemeingefühl in Hermann's *Handbuch d. Physiol.*, Leipzig, 1879, III, Part 2, 320f. and also G. T. Ladd. *Elements of Physiological Psychology*, N. Y., 1887, 347f.

[13] *Cf.* A. Winkelmann. *Handbuch der Physik*, Leipzig, 1906, III, Part 1, 489 & 505.

and that they are therefore to be avoided. Titchener,[14] for example, remarks in discussing the Griesbach aesthesiometer, —"The points are of metal. This is a disadvantage as it introduces the temperature error."

It is difficult to discover in the literature the origin of the use of non-metallic materials to eliminate thermal sensations. Hall and Donaldson[15] in 1885, in discussing the influence of temperature in the perception of movement on the skin, say: —"It was soon found, however, that these limits overlapped, *i. e.* that an intermediate temperature of a metallic point could be so chosen that it would stimulate both heat and cold spots. We therefore had recourse to non-conducting cork points of such shape that the smooth and rounded edge of a right-angled triangle was applied to the skin transversely to the direction of motion. Thus the sensations of temperature appeared to be entirely eliminated." In 1887, Hall and Motora[16] quite casually speak of the use of hard rubber for the same purpose. In the manufacture of aesthesiometers, bone and ivory have also been employed as non-conducting materials to eliminate temperature sensations.[17]

The new set of weights in the Clark Laboratory conform to all the requirements which we have noted. They are hard rubber cylinders, 2½ inches in diameter and 1 inch high. Hard rubber can be purchased in round bars, just 2½ inches in diameter. The cylinders are cut off and hollowed out at one end to a depth of ½ inch; a wall at the sides ⅓ inch in thickness is left. A round brass plate, ⅛ inch in thickness, is inserted into this cavity and is held in position by two bolts and nuts which pass through holes bored in the top of the weight and in the brass plate. The heads of the bolts are counter-sunk into the top of the weight so that they are below the surface. A small number is stamped with a steel die on each weight for purposes of identification.

When the weights are calibrated, the brass plate is unscrewed and solder is melted on it until the rubber shell, the brass plate, the nuts, the bolts and the solder weigh just a little more than the desired amount. Some of the solder is then carefully scraped away until the desired weight is obtained.

[14] E. B. Titchener. *Experimental Psychology*. N. Y., 1901, I., Part 1., 382.

[15] G. S. Hall & H. H. Donaldson. Motor Sensations on the Skin. *Mind* O. S. X., 1885, 568.

[16] G. S. Hall & Y. Motora. Dermal Sensitiveness to Gradual Pressure Changes. *Amer. Jour. of Psychol.*, I., 1887, 74.

[17] *Cf.* A. Gemelli. Un nuovo estesiometro. *Atti. d Soc. Ital. d. Sci. Nat.*, LII., 1913, 193-200.

This present set of weights has been tested a number of times during a period of two weeks in which there have been great variations in temperature and humidity. In no case did any weight vary as much as 5 mg., a variation which is far within the limits of accuracy required in the lifted weight experiment.

Under experimental conditions these weights actually do not arouse temperature sensations. Even when placed for several hours on a very hot radiator or outdoors in air at 20 F., several subjects found that the hard rubber weights were only slightly cool and warm, whereas the brass weights were almost painfully cold and painfully hot. Presumably even unusual changes of room-temperature with the hard rubber weights will not arouse sensations of warmth or cold at all.

THE PSYCHOLOGICAL EXAMINATION OF CONSCIENTIOUS OBJECTORS[1]

By Mark A. May, Syracuse University

Psychological examining in the United States Army yielded a large body of results that may be considered as by-products. Most of these by-products will not appear in the official report which is now in the course of publication. In order that these incidental results be made available it has been thought wise to publish them as separate studies. The present study is a report made by the writer to the office of the Surgeon General, December 1918. It is an attempt to present in a summary fashion the available information concerning the intelligence, education, grounds of objection, social and political history of conscientious objectors..

The first psychological examinations of conscientious objectors were made in compliance with a letter from The Adjutant General of the Army dated March 6th, 1918. At that time there were no psychological examiners on duty at most of the camps. These first examinations were made by the psychiatrists and medical officers or by Special Boards. The reports of these early examinations are very diverse in character and extremely difficult to summarize. On April 22, 1918, Major R. M. Yerkes summarized these data in a report for the Secretary of War. The essential facts of this summary are embodied in this report. As soon as psychological examiners were on duty at most of the camps and cantonments a letter was sent out from the Surgeon General's Office, Division of Psychology, directing how the examinations should be conducted and the reports should be made. In June, 1918, a special examination for conscientious objectors was sent out to the camps. A copy of this examination appears in an appendix to this paper.

The data presented here are taken from about 30 reports sent to the Surgeon General's Office by the various psychological examiners. Some camps made several reports, others made one and some made none. The reports available for this summary cover about 20 camps and represent about 1000

[1] Published with the approval of the War Department.

objectors. This is a little less than one half of the total number of conscientious objectors in the army. From a careful examination of the available information the writer is of the opinion that the 1000 cases presented here is a fair sampling of the total number.

The intelligence of conscientious objectors, as measured by the army mental tests, is on the average above that of the white draft of the army as a whole. By the army mental tests intelligence is rated on a scale of A, B, C+, C, C—, D, D—. The following table compares the intelligence ratings of the white draft of the army and the conscientious objectors.

		White Draft[2] (94,000 Cases)	Conscientious Objectors (1,000 Cases)
A	"Very Superior"	4.1%	8.7%
B	"Superior"	8.0%	15.2%
C+	"High Average"	15.2	22.6
C	"Average"	25.0	24.8
C—	"Low Average"	23.8	16.8
D	"Inferior"	17.0	8.7
D—	"Very Inferior"	7.1	3.1

These figures show that 46.5% of the conscientious objectors are above the grade of "C" which is *Average*, while in the army as a whole only 27.3% are above "C." And conversely 28.6% of the conscientious objectors are below "C" while 47.9% of the army as a whole are below "C." Another interesting fact about this table is that the ratio of A & B men of the army as a whole to the A & B men of the conscientious objectors is about 1:2. Conversely this ratio for D and D— men is about 2:1. There is absolutely nothing in these data to justify the hasty conclusion that objection to war is due to the low mentality of the objector. It may be true that some objectors can be classed as fanatics, others may be said to be stupid, but it can safely be said that at least 97% of the conscientious objectors reported here have sufficient intelligence to know what they are doing.

Educational data were available on 880 objectors. The following table shows the percentages of these men who reached the various grades in school.[3]

[2] These figures are quoted from page 1076 of the Report of the Surgeon General to the Secretary of War, 1919.

[3] It is regretted that the educational data of the army as a whole are not available at this time. They will appear in "Psychological Examining in the U. S. Army," Memoirs of The National Academy of Sciences, vol 15 (In press).

Grade Reached	Per Cent.	Grade Reached	Per Cent.
0	100	H. S. I	42
1	99	H. S. II	29
2	98	H. S. III	24
3	97	H. S. IV	18
4	94		
5	88	Col. I	12
6	80	Col. II	9
7	70	Col. III	6
8	58	Col. IV	3
		Professional	5

It will be pointed out presently that about 50% of the men in this report are Mennonites. Less than 10% of the Mennonites went beyond the 8th grade. The 12% who reached college were either Socialists, Dunkards or Friends.

The religious denominations of 1060 conscientious objectors are as follows:

Mennonites (Old and New)	342
Mennonites (Defenseless)	13
Mennonites (Swiss)	12
Mennonites (Amish)	99
Mennonites (Old Amish)	88
	554
Friends (Quakers)	80
Brethren, Church of the	67
International Bible Students (Russellites)	60
Dunkards	37
Israelites of the House of David	39
The Church of Christ	31
The Church of God and Saints of Christ, Etc	20
Seventh Day Adventists	20
Methodists	14
Pentecostal Assembly	13
Catholics	9
Assembly of God	9
Missionary Church	8
Pentecostal Church of the Nazarene	8
Lutherans	8
Christians (Campbellites)	7
Christadelphians	7
Holiness	6
Apostolic Christian	6
Baptist	5
Brethren (Plymouth)	5
Church of the First Born	4
Congregationalists	4
Presbyterians	3
Zionists	3

Other denominations of less than 3 are: True Light Church,

Christian Science, Brethren in Christ, Baptists (Free Will) Evangelists, Baptist (Austrian), Baptist (United), Brethren (United), Christian Lights, Gospel Hall, Universalists. Stundists, Rosacrucians, Mazdazman, Brotherhood of Mystics. Ethical Culture Society. In addition to this list 55 cases are reported as having no denominational affiliations at all.

Attention has already been called to the fact that about one half of the conscientious objectors are Mennonites of one branch or another. The Friends, Brethren, Dunkards, International Bible Students (Russellites), and Israelites of the House of David constitute about 25% of the total. These taken with the Mennonites make up fully 75% of the conscientious objectors.

It is interesting to observe that all of these sects, with the possible exception of the Friends, believe in the literal interpretation of the Scriptures. As will be seen later their appeal is usually to the Bible and to their church creed. Moreover they are all more or less clannish. This is especially true of the Mennonites who live in colonies in the middle west. In some instances they constitute a large part of the population of one county.

The list of religions "organized prior to May, 1917 and well-recognized, whose creeds forbid participation in war" are relatively few. Such a list has been prepared by Mr. Hunt of the Census Bureau and is as follows:

Mennonites, 16 bodies (including the Amish);
Friends, (Quakers) 4 bodies;
Dunkards, three bodies;
Brethren, four bodies;
German Baptists; two bodies;
Churches of Christ;
Christadelphians;
United Zionists;
Assemblies of God;
Pentecostal Church of Nazarene;
Apostolic Holiness;
Fellowship of Reconciliation (not a religious sect);
International Bible Students Association (Not an organized sect).

The denominational lists have interest to the psychologist because in conducting an examination it was found necessary to know something of the religious background of the objector. (See the examination at the end of this article).

The grounds of objection to war are in general three, religious, social and political. The religious objector makes

his appeal to the Bible, church creed and to conscience. The social objector makes his appeal to individual freedom. The political objector usually bases his objection on the ground of alien citizenship. Out of 958 cases, 90% object on religious grounds; 5% on social grounds; 3% on political grounds and 2%on ethical grounds. It is quite obvious that the problem of the conscientious objector is a problem of dealing with religions.

In many cases records were kept of the specific objections which were made. Such records were available of about 500 cases. Object Because:

War is forbidden by church and creed	115
War is forbidden in the Scriptures in general	125
War is forbidden by Christ	95
War is forbidden by the commandment	60
War is forbidden by conscience	120
War promotes evil	16
War is wrong in and of itself	21
War is capitalistic and Imperialistic	10
War is getting autocracy	7
Have friends among the enemy	7
Not citizens of this country	6

Other objections not occurring very often but which are rather interesting are:

Objector believes that he will lose his soul and go to Hell if he should disobey his church and his conscience and go to war.

Objector says he cannot obey God and Mammon. Thus he could not obey God and the army sergeant or captain at the same time.

Objector refuses to use carnal weapons. Prefers the sword of the spirit.

Objector refuses to participate in war because it involves hatred toward the enemy.

Objector believes that war is punishment of the unjust and the just are, therefore, exempt.

Objector fears that the temptations of army life will lead him astray.

Objector refuses to be "yoked with unbelievers" and thereby jeopardize his chances of salvation.

Objector believes that war is legalized murder.

Objector refuses to use physical force to the extent of separating soul from body.

Objector promised parents and pastor that he would not fight and feels that he must remain true to his promise.

Objector says "war makes the rich richer and the poor poorer."

Objector doubts America's motives.

Objector says he was not allowed to vote on the war.

Objector says that by staying out he will encourage socialism in Germany.

The political affiliations of about 340 objectors were secured. Of these 340 there were 45 Republicans, 62 Democrats, 22 Socialists, 26 Prohibitionists, 5 Independents, 2 Progressives, 3 not citizens, 3 other parties, and 75 no political affiliation at all. It is interesting that about one half this list have no political affiliations. Nearly all of these non-voters are either Mennonites, Dunkards, Brethren, or I. B. S. A., or Friends. But this is not as significant as it seems to be since it has been pointed out that about 75% of all the objectors belong to these denominations. The writer is of the opinion that there is no causal connection between this apparent disinterest in political affairs and objectors to war. It should be remembered that some religious denominations actually forbid the communicants to take any part in politics on the grounds that they must keep "unspotted from the world."

As far as the information goes very few objectors belong to fraternal organizations. The reports show that more than 90% have no fraternal affiliations. The ten per cent that do belong are scattered over a large number of organizations. Speaking generally one might say that the average conscientious objector is a non-voter, belongs to no social organizations, is not interested in community enterprises. The writer has examined more than fifty objectors and has not found a single individual who had even been superintendent of a Sunday school or held a responsible position in the community.

At the time of the psychological examination the examiner in some instances asked the objector what kind of service he would be willing to render. Reports in 718 cases show the following choices:

Willing to accept regular military service	16
Willing to accept non-combatant service	160
Willing to accept farm furlough	275
Unwilling to accept any kind of service that is any way connected with the military machine	267
	718

The Board of Inquiry with whom the final disposition of these men rested did not in all cases give the objector the kind of service he wanted. It did, however, consider the objector's preferences. The following table is quoted from W. G Kellogg's book, The Conscientious Objector, page 127. It shows the final disposition made of 2100 cases.

Recommended for farm or industrial furlough.....	1500
Recommended for Friends' Reconstruction Unit....	88
Recommended for noncombatant service.....	390
Assigned to general military service (insincere)....	122
	2100

The factors determining the kind of service an objector was willing to accept are hard to find. They appear to be neither religious nor intellectual. There is little difference in the average intelligence ratings of the men choosing these various forms of service.

Many psychological examiners made an effort to get at the objector's occupational history. In many cases these data would throw some light on the case. The occupations of 888 objectors were obtained. The more frequent ones are listed here.

Farmers	548	Printers	11
Clerks	40	Salesmen	11
Laborers	27	Miners	10
Machinists	20	Chauffers	10
Students	24	Bookkeepers	6
Teachers	22	Engineers	5
Carpenters	17	Factory Workers	5
Ministers	16	Electricians	4

A total of 90 different occupations is represented by these 888 objectors. They run the whole gamut from accountants to zinc workers including one saloon-keeper and a gunsmith. The most striking fact about this list of occupations is the large number of farmers. This in turn is due to the preponderance of Mennonites of whom 90% are farmers.

The available data on the birth and ancestry of conscientious objectors show that out of 720 cases 90% are native born. No accurate information is available on the ancestry of objectors but what is at hand seems to indicate that approximately one third were of American parentage, one third German, and one

third English, Scotch, Russian and Scandinavian. The writer is extremely doubtful if parentage or birthplace has anything to do with an objector's refusal to take part in war. The large percentage of German parentage can easily be accounted for by the fact that most Mennonites are of German descent. Conscientious Objectors cannot be intelligently disposed of by simply calling them pro-German.

The degree of sincerity of a conscientious objector is a thing almost impossible to determine. It is difficult enough to find a test that will identify him, much less to find a measure of his sincerity. Of course, it was not the business of the psychologist to pass final judgment on the sincerity of an objector. But since the psychologist was often called upon to testify in courts-martial proceedings, to pass on an opinion to company commanders, and to furnish testimony to the Board of Inquiry it was necessary that he be able to formulate some opinion as to the sincerity of the objector. One of the chief purposes of the examination herewith attached was to aid the examiner in making up his mind. This examination seeks (1) to determine the objector's intelligence and to find out if he has any mental abnormalities. (2) His educational and occupational history is gone into with a view to getting at his interests. (3) Questions are asked about his church creed and his religious experiences to see how well informed he is. The assumption is that if an individual bases his objection to war on a church creed he ought to know something about it. (4) His moral habits and his social outlook are reviewed. In short the psychologist attempts to get as many lines as possible on the objector's character and social trustworthiness. All this evidence is put together when the psychologist makes his report. Such questions as "What would you do if a German should attack your Grandmother?" are carefully avoided. Questions of this sort are not tests of sincerity and show poor judgment on the part of the examiner. The non-resistance issue was not raised. It leads nowhere.

The Board of Inquiry passed on approximately 2100 cases.

The disposition of these cases was as follows:

Found to be sincere objectors and recommended for farm or industrial furloughs	1500
Found to be sincere objectors to combatant but insincere as to noncombatant service	219
Found to be sincere objectors of combatant but willing to accept noncombatant service	156

Found to be sincere objectors to combatant service but willing to work in reconstruction hospitals.....	15
Found to be insincere both as to combatant and noncombatant service	122

(This table is quoted from Mr. Kellogg's book cited above).

Unfortunately no reports are available showing how well the psychologists' recommendations correlated with the findings of the Board. It was the writer's experience in examining objectors that after carefully applying the examination little doubt remained in the mind of the examiner as to the objector's sincerity or insincerity.

Some general conclusions regarding the psychology of conscientious objectors may now be given. In the first place conscientious objection to war cannot be attributed to inferior intelligence. While it may be true that some religious sects are below the average intelligence of the army, yet they are not far enough below it to have any effect on objection to war. Taken as a whole the conscientious objectors are far superior in intelligence to the white draft of the army taken as a whole. Neither can objection to war be laid at the door of abnormal mental states. It is true that some objectors exhibit mental instability, possibly some are religious fanatics, yet a great majority of them are sane. Moreover conscientious objection to war is not a special case of the "blind leading the blind." Many objectors adhere rigidly to their creeds and follow the instructions of their pastors and so on, but many of them are keen thinkers. They are quick in debate and logical in their arguments. Mental questioning rather than unquestioning is more often the cause of objection.

From a survey of the available data three types of conscientious objectors stand out rather clearly. First there is what may be called the *religious-literalist* type. To this class belong most of the Mennonites, Dunkards, some branches of the Brethren and many of the obscure denominations mentioned above. If an objector of this type were picked at random he might be characterized as follows: He is a Mennonite, born on a farm in a middle Western community. His parents together with others of their faith settled there many generations ago. He went to the country church school. There he was taught the Bible and some simple rules of living. He quit school at the 8th grade and went to work on the farm. He attended church more or less regularly. He was taught to respect his minister and his views of life. He perhaps read

the county newspaper and the Bible, outside of this he has done little reading. His information is limited to the happenings of his own community. He knows that we are at war with Germany but has no idea why. He has no social vision, no national consciousness. So when creed, minister, parents and friends tell him that war is wrong and that he must not fight, what could be expected of him? For a man like this not to be a conscientious objector would violate all the laws of heredity and environment that operate to make men pursue certain courses of action. This type of man constitutes fully 75% of the conscientious objectors.

Secondly, there is the *religious-idealist* type. His appeal is not to church or creed, although he is usually a church member. He appeals directly to conscience and to the teachings of Jesus. He was probably born in a city or an enlightened community and is probably a high school or college graduate. Some men of this type hold advanced degrees from large universities. He is a true Christian and has for years tried to follow closely the example and teaching of Jesus. He is very intelligent, in fact, he may measure up with many officers on the army intelligence scales. He is a thoroughgoing idealist and is entirely unwilling to sacrifice his ideals by resorting to expediency. He perhaps does not have the support of his pastor or friends. He has but one principle of life and that is the principle of love. He believes in applying this principle even in dealing with his enemies. In most cases he refuses to make any compromises and for this reason he is the type usually found in the Disciplinary Barracks. The fault with his type is not that he does not have social vision, but that he has too much.

Thirdly, there is the *socialist* type. He is likely to be well educated and very intelligent. He is well informed on the vital social and economic problems of the day. His patriotism knows no national limits. Here again it is not that he is not patriotic, but he is unusually so. He believes in the Brotherhood of Man, a United States of the World, a League of Free Nations, an ideal state of society in which all shall have equal rights. He is not willing to fight the proletariat of Germany just to get at the ruling classes. He may even believe in the use of force and in fighting, but it is capitalism and imperialism that he wants to fight. The problem with him is that he entirely fails to appreciate "national" problems. The last two classes of objectors constitute about 25% of the whole.

APPENDIX

EXAMINATION OF CONSCIENTIOUS OBJECTORS

This examination outline is supplied to every examining staff in order that it may be used as seems desirable. Its use is not ordered but it is believed that it will greatly assist examiners who lack experience.

Purpose:

1. To determine the degree of intelligence of the conscientious objector, with a view to finding out whether his objections are due to any abnormal mental conditions.
2. To obtain the *specific grounds* of the conscientious objector's objections, and to see how well grounded he is in them (not how consistent).
3. To ascertain the extent of the conscientious objector's information concerning the history and tenets of his faith, and of the organization (if any) to which he belongs. This is for the purpose of ascertaining his objections in somewhat the same way as that employed by Personnel Officers with reference to skill in occupations.
4. To determine the degree of the conscientious objector's objections; just what he is and is not willing to do in the war; e. g., wear the uniform, take the military oath, enter non-combatant service or non-military service, reconstruction, etc.
5. To determine the degree of his sincerity.

Note:—The following examination is worked out with a view to covering as many kinds of cases as are likely to occur. It is not necessary that the entire examination be given to any one man. As soon as the examiner has reached the point where he can be reasonably sure that he has sufficient information to satisfy the above requirements, the examination should cease.

I. *Mental Age Examination.*

If the subject speaks English, use Stanford-Binet or Point Scale; if illiterate or non-English speaking, use Performance Test. In any case give as many tests involving verbal responses as possible. Write down all responses in full. Watch carefully for all signs of malingering, for responses that indicate a psychopathic condition, and for responses that have a definite religious trend. Calculate the mental age at once, and let your subsequent questions be governed by the degree of the subject's intelligence.

II. *Personal and Family History.*

1. Are your parents living? If not, state cause of death.
2. Any mental disease or peculiarities existing in family?
3. What blood relationship exists between parents?
4. Have you ever been troubled with sleeplessness, excessive worry, discontent, or any nervous disability?
5. Have you ever had any serious disease or physical defects?

III. *Educational History.*

1. Where did you go to school? How long?
2. How old were you when you quit? Why did you quit?
3. What subject interested you most?
4. How did you get along in school?
5. What books and magazines do you read most now?

IV. *Occupational History.* (Important, if subject is Socialist.)

1. What was your occupation just before joining the Army? How long did you work at it?
2. How many different jobs have you had in your life? Where? Why changes?
3. Do you belong to a trade union? Why or why not?
4. Did you ever participate in a strike?
5. Did you earn enough money to live comfortably?

V. *Religious History and Practices.*

1. What is your denomination? What branch? How long have you been a member? Do you have certificate of membership?
2. To how many different denominations have you belonged? Why changes?
3. When, where and how converted? How many times? (Note especially specific religious experiences, such as visions, divine messages, etc.)
4. What is religion of father, mother, brothers and sisters?
5. Have you ever studied for the ministry? Every preached? How long? Where?
6. Do you read the Bible daily? How much? What parts interest you most?
7. What religious books other than the Bible do you read?
8. How often do you go to church?
9. How often do you pray? For whom and for what? Do you pray for the Kaiser?
10. Have you ever got direct answers to prayer?
11. Did you attempt to convert others to your religion?

VI. *Moral History and Habits.*

1.	Are you a sex offender?	(Reformed?)	(Why?)
2.	Do you smoke or drink?	(Reformed?)	(Why?)
3.	Do you swear?	(Reformed?)	(Why?)
4.	Do you gamble?	(Reformed?)	(Why?)
5.	Do you contract debts?	(Reformed?)	(Why?)
6.	Do you pay debts?		
7.	Do you lie?	(Reformed?)	(Why?)
8.	Do you steal?	(Reformed?)	(Why?)

9. What do you consider your worst habits?

VII. *Social History.*

1. Have you ever held an office in your community or church?
2. Do you belong to any secret societies? Ever held office?
3. Are you interested in community enterprises? (Good roads, etc.)
4. In what ways have you wanted to make your community better? Or are you satisfied to let every man take care of himself?
5. Have you ever been before a court on a charge? In jail?

VIII. *Grounds and Degree of Objection.* (If the objections are on religious grounds, follow A.; if on social or political grounds, follow B.)

A *Religious Objections.*

1. Degree of objection.
   a. Do you object to all wars or only to this war?

b. Would you take the military oath?
wear the uniform?
work in reconstruction?
do non-military work, such as farming?
enter a non-combatant military organization?

2. Specific objections.
   a. Do you object to war because your church forbids it?
      i. Why does it forbid it?
      ii. How long has it forbidden it?
      iii. Have any members of your church ever taken part in war?
      iv. Do you know any of your fellow-members who have been drafted? What did they do?
      v. Would you take part in a war to defend your religion?
   b. Do you object to war because the commandment says "Thou shalt not kill?"
      i. Do you literally obey the other commandments?
      ii. Do you consider all killing murder?
      iii. How do you account for divine sanction for war in the Old Testament?
   c. Do you base your objection on passages of Scripture found in the New Testament? What are they?
      i. Do you live up to the teachings of Jesus in all respects?
      ii. What do you think Jesus would do if he were drafted? Why?
      iii. What is your favorite passage?
   d. Do you object to war because you think that if you went to war and were killed, you would go to hell?
   e. Do you object to going to war because you are chosen of God not to fight with carnal weapons?
      i. Do you think it is wrong for me to fight?
      ii. How do you account for this war in God's plan for the world?
   f. Do you object to war because war involves hate? Makes deception legitimate? Impedes thereby the coming of the Brotherhood of Man?

3. Information concerning the History and Tenets of his Faith.
   a. How many members has your church as a whole? where mostly?
   b. Who was the founder of your faith?
   c. When were they first called by their present name?
   d. What sacraments do they believe in?
   e. Is the creed of your church written down outside the Bible?
   f. What does your church do to discipline disobedient members?
   g. What is its attitude towards the civil law?
   h. Does your church believe in education?
   i. Does your church co-operate with other religions? Y. M. C. A.?

1. Degree of objection.
   j. Does your church permit marriage outside the church?
   k. Does it believe in the Resurrection of the Body?

B. *Objections on Social, Economic and Political Grounds.*

a. Do you object to all wars or only to this war?
b. Would you take the military oath? wear the uniform? work in re-construction? do non-military work, such as farming? enter a non-combatant military organization?

2. Specific objections.
   a. Do you object because this is a capitalistic and materialistic war? In what ways?
   b. Do you object because the war is not worth the expense?
   c. Do you object because the war is not getting Democracy, or rather is bringing about a German Autocracy, in this country?
   d. Do you object to being drafted for a defensive war, or is your objection to being drafted and sent to Europe?
   e. Do you object because the people did not vote on the war question?
   f. Do you object because you refuse to be swayed by mob psychology and prefer to be in the minority?

3. Questions to determine how well grounded subject is in his views.
   a. How long have you held these views?
   b. What social organization are you a member of? How long? What is its peace program?
   c. How did you come to be a Socialist?
   d. What books and magazines do you read?
   e. As a Socialist what do you consider your personal duty towards the war?
   f. Do you believe that by staying out of the war you encourage your social brethren in Germany in a social revolution? Why has there not been such a revolution, already?
   g. Do you believe in police force and enforced arbitration?
   h. Do you claim protection under the law, the right to vote, freedom of speech? Do you think that such liberties were obtained by war?
   i. Are you in accord with President Wilson's peace aims?
   j. Why do not all Socialists oppose the war?
   k. What has been the attitude of Socialist parties here and elsewhere on past wars?
   l. What efforts do you make to spread the Socialist view?

4. Questions to ascertain the extent of subject's information concerning Socialism.
   a. What is the Socialist view as to the cause of the war?
   b. What are the Socialist war aims? Peace program?
   c. What practical things have the Socialists done to end the war?
   d. What is the attitude of the German Socialist towards the war?
   e. Name some prominent Socialists here and abroad and give their viewpoints.
   f. What is the Russian peace formula? Attitude of Germany, England and France thereto?
   g. What do you know about the Stockholm conference?
   h. To what extent has the war furthered the cause of socialism here?

THE VOWEL CHARACTER OF FORK TONES

By A. P. Weiss, Ohio State University

INTRODUCTION

I. Experiment with Elementary Students 168
II. Experiment with Laboratory Students 173
III. Experiment with Teachers of Deaf Children 177
IV. The Distribution of Vowels 180
V. The Consistency Between Vowels 183
VI. The Effect of Practice on the Consistency 185
VII. The Consistency Between Vocality and Vibration Rate 186
VIII. Comparison with Work of Koehler 187
IX. Discussion of Vocality 189
X. Summary and Conclusions 191

Introduction. This set of experiments was suggested by a series of observations made by Koehler (2) in 1910 while he was investigating the motions of the tympanic membrane during auditory function. In the analysis of the photographic records of the vibrations secured by this method it was found that when the vowel ō (fold)[2] was sung at about 500 vibrations. the form of the sound wave approached a pure sine curve. At about 200 vibrations the sound wave for ō (fold) became very complex, but the partial corresponding to the frequency 500 became prominent while the fundamental 200 almost entirely disappeared. This fact led Koehler to conclude that pure tones had an attribute or property which he designated as '*Vokalcharacter*' and that this attribute changed in quality at intervals of about an octave. Thus the '*Vokalcharacter*' of the tone 265 is ū (soon), that of 526 is ō (fold) and that of 1066 is ā (far).

In going over Koehler's results in a limited way it was not possible to differentiate these vowels unequivocally. From his familiarity with the work of Meyer (5) the writer wondered whether Koehler had not merely made a more intensive study of what Meyer had described as 'quality' and which

[2] The designation of vowels will be in the Revised Scientific (National Education Association) Alphabet Key. For those unfamiliar with this alphabet short English words in parentheses indicate the sound of the vowel. For reference to key see (1) in list of references.

was regarded as a continuous series ranging from the 'mellowness' of the lowest tones to the 'shrillness' of the highest tones with intermediate degrees of mellowness and shrillness for the intermediate vibration frequencies.

Experiments were planned bearing on this question. One investigation aimed to determine the vowel character as judged by untrained or partially trained subjects to get some idea of the acuity of discrimination in general. The orientation seemed desirable so as to plan a second experiment using refined apparatus and specially trained subjects with an intensive technique. The experiment with the untrained subjects was started and finished while work on the intensive apparatus was progressing. The latter was brought to a stop by the war which made it impossible to secure the quality of tuning forks necessary for the work. The writer regrets that the intensive results as originally planned cannot be included in this study; but since there is no prospect of getting the forks and the apparatus within a reasonable time, the qualitative results are presented separately for the benefit of those who already have the facilities and interest for the quantitative aspect.

Statement of the Problem. The experiments were designed to reveal the extent to which tuning fork tones resembled the vowels used in language. As a supplementary phase of the experiment there was also opportunity for considering the terminological aspect of the psychology of audition. The teaching of the attributes or simplest discriminations in hearing presents considerable difficulty to students, a difficulty which is perhaps only due to ambiguous or unfamiliar terminology. The divergences between musical and psychological practices in describing auditory phenomena are so great that some investigators think it hopeless to try to reconcile them. Among acousticians themselves the situation is not much better. A random list of the terms used by various investigators to describe the attributes of audition are: pitch, quality, tonality, vocality, tone-color, intensity, duration, volume, brightness, extent, clearness and local signature. Does this list of a dozen terms mean that twelve attributes may be discriminated? No investigator would maintain this. Does any particular term (say quality) mean the same for all investigators? Again the answer is negative. Indeed the pessimistically inclined would maintain that any one of the dozen terms may mean any one of the other eleven.

Surely if experts cannot agree, then to learn the opinion of the naïve subject cannot add appreciably to the confusion. Whether the ability to detect a resemblance to vowels in fork

tones is best described by the term quality, vocality, tone-color, brightness, volume, or any other term, will be determined in part by the ease with which untrained subjects can consistently discriminate vowels. If more consistent results are secured when the subject is asked to discriminate the vowel character than when he is asked to discriminate the brightness, quality or tone-color, supposing these all refer to the same fundamental attribute, then obviously vowel character or vocality is much the better term to use.

To test the value of a term, untrained subjects may often be used more effectively than trained subjects with special habits and prejudices toward a terminology developed under circumstances deviating very widely from the average class room conditions under which psychology is taught. Because of the lack of agreement on terminology no attempt will be made to determine whether the discriminations made by the subjects are attributive or cognitive in character. It is maintained only that an habitual or practical method of discrimination is demonstrated to the degree that there is consistency in the vowels that are assigned to the various fork tones.

Since performing these experiments Meyer (4) has suggested the term *vocality* as a substitute for his earlier term *quality* and as a shortened form of *vowel character*. It is in this sense that we shall use the term vocality in this article. Titchener (8) seems disposed to adopt the term vocality in the sense in which Meyer suggests, but Titchener has retained the term *volume* which is regarded as an attribute independent of the vocality. Rich (10) regards *pitch* and *volume* as independent attributes, but does not indicate the relationship between vocality and volume.

Simply stated our problem is to determine how consistently relatively untrained subjects can discriminate vocality in tuning fork tones and what utility the term vocality has in describing a fundamental fact in audition.

I. Experiment with Elementary Students

Class Experiment

A class experiment was given the last week in November, 1913 at Ohio State University to 150 students mostly freshmen and sophomores taking the course in elementary psychology.

Experimental Method. Ten Koenig tuning forks mounted on resonance boxes were placed at the front of the lecture room. The vibration rates of these forks were 128, 256, 320,

384, 512, 640, 768, 896, 1024, 1152 double vibrations. The subjects were instructed as follows.

Each fork will be sounded twice. The first time it is bowed merely listen, eyes closed, and form a provisional judgment as to which vowel in the English language the tone most nearly resembles. The second time the fork is sounded make a final judgment and record a short English word which contains the vowel sound. The list of vowels and words on the board are to be regarded as merely illustrative. They do not include all the vowel sounds in the language nor all that may be used in reporting the vowel character of the fork tones.

The following is the list of vowels and words that was placed on the board:

e as in feel, *a* as in sale *o* as in tone *u* as in moon,
a as in far, *oi* as in boy, *yu* as in cure.

The tones lasted about three seconds. The recording was done on a narrow strip of paper, beginning at the bottom. As soon as the word was written it was folded out of sight. The next word was placed on top of the folded portion and it in turn was folded down. In this way there was no copying or referring back to previous judgments. In the presentation high and low forks alternated indiscriminately.

The term *series* as used in these experiments means a single presentation of each of the ten forks. A *discrimination* or *judgment* means that a fork tone has been sounded one or more times and that the subject has reacted to the tone by reporting, either orally or in writing, that vowel which in his opinion most nearly resembled the fork tone.

While performing the experiment it was noticed that the reactions were delayed and hesitating. Judging from the facial expressions the discriminations were difficult to make during the earlier part of the experiment but toward the end the reactions were more prompt.

After one week the experiment was repeated. This repetition is designated Series B; the original presentation as Series A. The second series was not expected by the students. The repetition was made to determine whether a second series would reveal greater uniformity and thus indicate some practice effect. The method was the same as for Series A. The results of both series are recorded in Table I. To enable direct comparisons with the subsequent experiments in which the number of subjects and the number of series are different, all results have been converted into percentages.

TABLE I

CLASS EXPERIMENT. TWO SERIES

First Series = A. Second Series = B.

One hundred and fifty elementary students. The values in the table represent percentages. Thus the first value 48 means that for the fork 128 in Series A, the vowel ū (soon) was reported in 48 per cent of the 150 judgments.

Forks / Series	128 A	128 B	256 A	256 B	320 A	320 B	384 A	384 B	512 A	512 B	640 A	640 B	768 A	768 B	896 A	896 B	1024 A	1024 B	1152 A	1152 B
ū (soon)	48	86	10	18	12	20	8	16	3	0	6	0	5	..	10	0	8	0	2	0
iū (cure)	6	0	10	0	10	0	4	0	6	0	11	0	8	..	8	0	6	0	5	0
ō (fold)	8	12	14	34	11	19	8	34	10	0	7	0	8	..	6	0	5	0	7	0
oi (boy)	4	0	7	0	9	0	10	0	14	0	10	0	14	..	8	0	5	0	8	0
ä (far)	3	0	4	12	9	31	9	43	3	3	4	2	9	..	5	0	6	0	6	0
ai (fine)	1	0	3	0	9	0	15	0	24	29	22	25	18	..	14	33	16	42	18	31
ā (they)	4	0	22	34	9	27	14	7	10	35	7	20	10	..	14	34	9	12	12	13
ī (see)	15	0	15	0	14	0	15	0	14	33	14	50	15	..	17	33	21	46	18	53
failed	11	2	15	2	17	3	17	0	16	0	19	3	13	..	18	0	24	0	24	3

Last line 'failed' refers to the cases in which no judgment was given.
Average failures for Series A = 17 per cent; Series B = 2 per cent.
Average number of vowels assigned per fork: Series A = 8; Series B = 3.
In Series B, fork 768 was not available.

Table I. In Series A the failure to assign a vowel to the fork tones was nearly eight times as great as in Series B. The 17 per cent failures in Series A are not excessive however. If the same subjects had been asked to discriminate between ten degrees of saturation in colors, irregularly and successively presented, the failures probably would have been greater. That the failures decreased to 2 per cent in Series B indicates a rapid increase in skill with practice. In Series A the forks 128, 256, 512, 640 and 1024 have all of the eight vowels assigned to them, but some one of the vowels was used in more than one-fifth of the total number of judgments. In Series B the average number of vowels per fork was only three and every fork except 320 has more than one-third of the judgments given to some one vowel. This again implies a rapid increase in the uniformity of the reactions.

Nature of the Vowel Reaction. In Series A the percentage of ī (see) for all the forks was unusually large. The experimenter could not at first account for this fact. By bowing the forks gently the tones are so pure that no unpracticed subject would detect overtones and since the percentage was practically the same for all the forks it did not seem likely that the difficulty was in the source of the sounds.

It occurred to the experimenter that the subjects were not

only discriminating the particular vowel character of the tone but were also introducing an additional ĭ (see) vowel because they did not pronounce the vowel character of the fork tone orally. If for instance a fork whose vowel character resembles ō (fold) is sounded and an attempt is made to pronounce or sing the *o* silently, some consonant will be spontaneously supplied by the whispering and any vowel component of this consonant would be especially emphasized because of the instructions to differentiate the *vowel* character. Thus if the consonant *h* is supplied to the *o* to make *ho,* the *h* would not be differentiated as *h* because of this emphasis on the *vowel* character. If *h* is regarded as a semi-vowel, its silent pronunciation would resemble the oral pronunciation of *heee* and this in connection with the emphasized vowel *o* would give *heeeooo.* The *heee* would form a part of every fork tone and thus increase the frequency of the ĭ (see) reaction for all the forks.

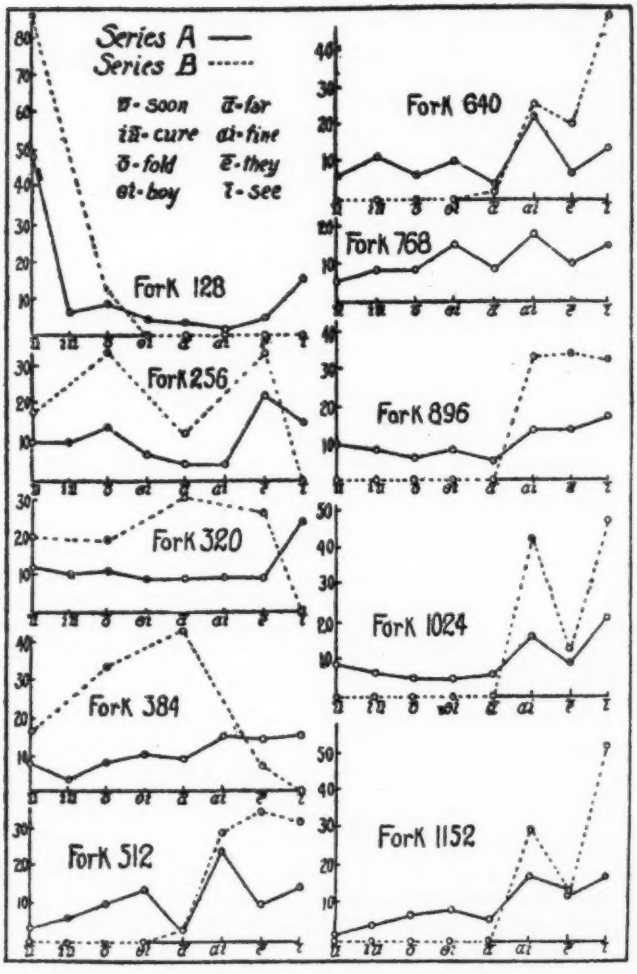
To restrict the analysis to the vowel character only, the experimenter, before Series B was given, called indirect attention to the consonants by stating that the vowel character of the fork tones may often be more easily analyzed if the subject will add consonants to the vowel sounds as is done in language. Thus we have pop, ish, peep, ho, ah, chirp, etc. It was hoped that by indirectly introducing the consonants their effect would be abstracted from the discrimination of the vowel character. The device seems to have been successful since the anomalous ĭ (see) distribution of Series A is absent in Series B. It was also pointed out that the sounds oi (boy) ai (fine), were not single vowels but diphthongs. This resulted in the elimination of oi (boy) and a restriction of ai (fine) to fewer forks.

Curves of Table I. The relation between Series A and B with respect to uniformity is best indicated by the curves. Each of the ten forks is plotted separately for both series. The vowels given on the base line are in the same order for all the forks.

If the vocality is independent of the vibration rates, the curves should approach the equation $y = k$ as a limit, approximately as is shown by the Series A curve for fork 320. Considering the Series A curves as a whole however, they would not be classed with the horizontal line type of curve. Series B is still further removed from the straight line type and the curves begin to give evidence of having definite maxima which in general augment the maxima of Series A. The maxima are toward the left for the lower forks; nearer the

CURVES I

ELEMENTARY STUDENTS. CLASS EXPERIMENT. VALUES TAKEN FROM TABLE I. EACH FORK PLOTTED SEPARATELY. VERTICALLY ORDINATE REPRESENTS PER CENT. VOWELS INDICATED ON BASE LINE



center for the intermediate forks; and toward the right for the higher forks.

The order of the vowels follows the series given by Koehler (3): *m*, ŭ, ŏ, ā, ē, ī, the diphthongs not given by Koehler were interpolated.

Flexibility of the Vowel Series. In brief supplementary experiments not reported in this article it was found that when the subject is asked to determine the vowel character of a series, he usually assigns ŭ (soon) to the lower tones, and ī (see) to the higher no matter what the range of the series may be. There seems to be a tendency to condense or expand the vocality series to conform to the vibration limits of the tones used in the particular experiment.

If more intensive work will substantiate this it would mean that vocality is only partly dependent upon the absolute vibrations. Builders of musical instruments have adopted this partial dependence in the principle of 'voicing.' An organ builder may take two reeds both of 512 vibrations and voice one of them mellow and the other shrill, but he cannot voice the 512 reed so that it will be as mellow as a 128 or as shrill as one of 2048 vibrations. Similarity in voicing is restricted to a limit beyond which it is impossible to go. More space is devoted to this phenomenon under IX Discussion of Vocality.

II. Experiment with Laboratory Students

Individual Experiment[3]

To determine how the uniformity in vocality discrimination increases with subjects somewhat better trained in psychological observation, twenty-five series similar to those given the elementary class were given to six students taking the course in experimental psychology and two instructors in the department.

The ten forks were placed on a table in a room free from noise or other distractions and the subject sat about three feet away with his back to the forks. Before beginning, the nature of the experiment was explained and the subject instructed as follows:

The pure tone from the fork is to be analyzed as to its resemblance to the vowel sounds used in language. If a fork tone does not resemble any

[3] Mr. R. F. Bird, recently Fellow at the Carnegie Institute of Technology conducted the experiments with the laboratory students. His critical and painstaking tabulation of their data made it possible to add his results to those of Experiments I and III.

vowel with which you are familiar, the vowel or combination of vowels which most closely approximates the tone is to be given. The fork tones will be repeated as often as desired to enable you to make a judgment. The series of vowels on the card which has been given you is not to be regarded as complete, but only as indicating some of the sounds. Any other vowels may be used. If a vowel character seems to resemble two vowels, this is to be indicated by giving both vowels.

The following is the list of vowels and words that were placed on the card:

m as in hum, *a* as in ball *u* as in cue, *ʌ* as in sale,
e as in feel, *o* as in tone, *i* as in high, *ɑ* as in father,
i as in sit.

when the subject stated that the fork sounded equally like two vowels, as û-ō for instance, this was counted as one-half for each of the vowels. Such double vowels occurred in less than one per cent of the total number of discriminations, and since the number of possible vowel combinations is large, each combination occurred so few times that no attempt was made to tabulate them separately. The most frequent combination ā-ē occurred only fourteen times.

Five series were given at a single sitting of about a half hour. The order of sounding the forks was varied for each series so the subject could not anticipate the next tone. The recording was done by the experimenter on previously prepared data sheets, the subject merely calling out the vowel and word.

When the first five series had been finished for the six subjects, the results were tabulated and it was found that all the vowel sounds that had been given could be grouped under the following vowels:

um as in hum	*ʌ* as in sale	*a* as in ball
u as in cue	*i* as in sit	*e* as in met
o as in hoe	*i* as in high	*a* as in cat
ɑ as in father	*e* as in feel	*u* as in full

The eight vowels in the first two columns were placed on a new card. The four vowels in the last column were omitted because they had been reported in less than two per cent of the cases. The revised card was then used for the rest of the experiment and the subject was requested to restrict his judgment to the vowels given on the card unless none of these were adequate either singly or in combination to express the vocality. In justification of this restriction it was found that the first eight vowels were in common use by all subjects including those who made use of the four vowels omitted.

During the early trials the subjects reported that the num-

ber of vowels at their disposal seemed too limited to describe all the fork effects. This difficulty was partly due to the unequal emphasis on the intensity and duration. Thus e (set) ē (they) are the same vowel differing only in length. A little practice however, soon enabled the subject to make due allowance for such variations but the more critical observers maintained to the last that when, for instance, two different forks were both reported to have the ō (fold) character this did not mean that the two *o*'s were the same. It implied only that the differences between the *o*'s could not be described in a consistent and satisfactory manner. Under IX Discussion of Vocality, further consideration is given to this limitation in the vowel analysis of fork tones.

TABLE II

EIGHT LABORATORY STUDENTS—TWENTY-FIVE SERIES FOR EACH SUBJECT—TOTAL OF 200 DISCRIMINATIONS FOR EACH FORK—THE VALUES IN THE TABLE REPRESENT PERCENTAGES—THUS THE FIRST VALUE 55 MEANS THAT FOR THE FORK 128 THE VOWEL UM (HUM) WAS REPORTED IN 55 PER CENT OF THE 200 DISCRIMINATIONS.

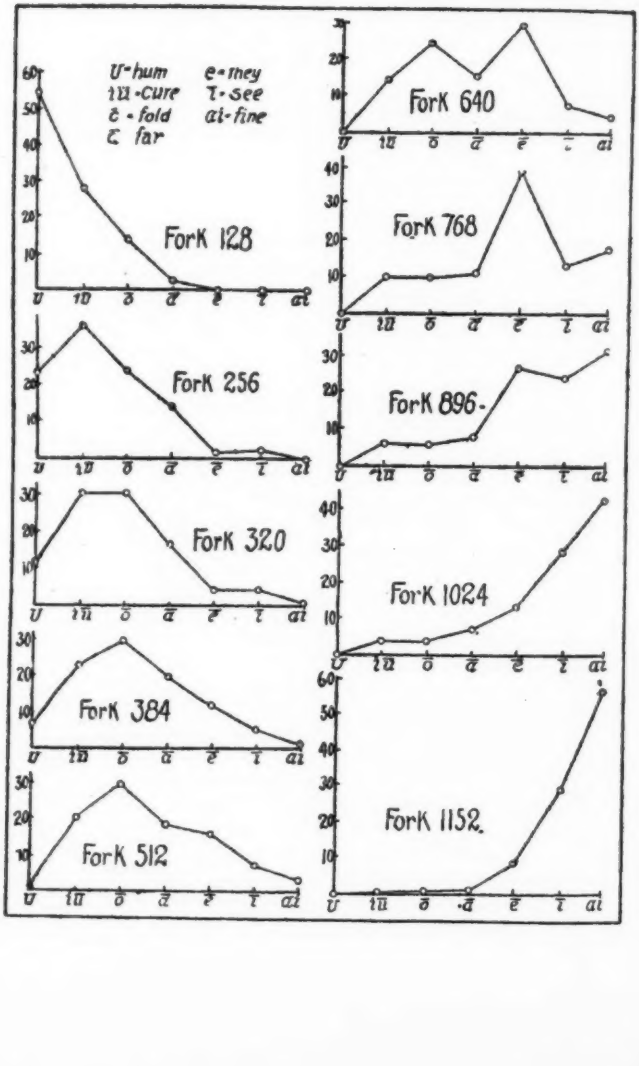
Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	55	23	12	7	1	1	0	0	0	0
iu (cure)	28	36	31	23	21	15	10	5	4	1
ō (fold)	14	24	31	30	30	25	10	5	4	1
ä (far)	3	14	17	20	19	16	11	7	7	2
ē (they)	0	1	4	12	17	30	39	27	13	9
ɪ (see)	0	2	4	6	8	8	13	24	29	30
ai (fine)	0	0	1	2	4	5	17	32	43	57

Table II. There are no failures since each subject was free to have the tone repeated until he was able to assign some vowel. The percentages of any fork when read from top to bottom, or for any vowel when read from left to right, form a much more uniform series than for Experiment I with the elementary students. While the maxima are not as high as for Series B in Table I the geometrical relations between the series are more clearly shown. The maxima begin at one end for the lower forks (55 for 128) passing toward the center for the intermediate forks (30 for 512) and ending at the other extreme for the higher forks (57 for 1152).

The fork 640 seems to have two maxima, one at ō (fold) and the other at ē (they). This however does not interrupt

CURVES II

LABORATORY STUDENTS—INDIVIDUAL EXPERIMENT—VALUES TAKEN FROM TABLE II—EACH FORK PLOTTED SEPARATELY—VERTICAL ORDINATE REPRESENTS PER CENT—VOWELS INDICATED ON BASE LINE.



the progression except in a way that might be expected when the intermediate vowels are less clearly differentiated.

Curves for Table II. The plan of plotting the curves is the same as for Curves I. The graph for fork 128 has its maximum at the extreme left; for fork 1152 the maximum is at the extreme right; and for the intermediate forks it approaches either the left or right according as their vibration rates approach the upper or lower limits of the series.

On the whole the curves show a much more orderly progression than those for Experiment I. This would be expected from the more extended practice of the subjects and the change in the order of the vowel progression. In Experiment II the diphthong ai (fine) is placed after instead of before the vowel ī (see) as in Experiment I. While phonetically *ai* (fine) is a combination of ā and ī, yet since it ranks third in the order of frequency it seems probable that it is perceptually a unit for most subjects. However, the difficulty in placing this diphthong seems to indicate that there is a partial analysis.

III. Experiment with Teachers of Deaf Children

Class Experiment

To determine how consistently the vocality of fork tones could be discriminated by subjects trained in the analysis of vowel sounds, a class experiment similar to that given in Experiment I was performed before thirty teachers of the State School for the Deaf[4] at Columbus, Ohio. These teachers taught oral speech to children of all degrees of deafness. To detect and correct speech errors in those who cannot hear their own voices requires special ability in the analysis of vowel character and this ability should manifest itself even though the vowel character of the fork tones varies so considerably from the vowels of actual speech that the teachers are unable to use their special training to its limit.

The experimental method was practically the same as in Experiment I. Ten series were given instead of only one. The forks were not seen by the subjects. The tones were presented at random, each fork being sounded twice,—the first tone for orientation, the second for final discrimination. The subjects were instructed to listen to the tone and then record a word, the vowel of which resembled the fork tone. If no

[4] The writer takes this opportunity to thank Superintendent B. H. Jones for his interest and suggestions, and the teachers for their patience and conscientious effort during a trying hour.

vowel could be assigned nothing was to be recorded. Data sheets, ruled in squares with each square numbered were provided. At the top of the sheet were given the vowels:

um as in hum, *a* as in father, *o* as in tone, *a* as in cat
a as in sale, *i* as in fine, *u* as in cute.

The subjects were to regard this list as merely illustrative. Vowels not in the list were to be used whenever necessary.

TABLE III

THIRTY TEACHERS AT THE STATE SCHOOL FOR THE DEAF.—TEN SERIES—TOTAL OF 300 DISCRIMINATIONS FOR EACH FORK—THE VALUES OF THE TABLE REPRESENT PERCENTAGES—THUS THE FIRST VALUE 28 MEANS THAT FOR THE FORK 128 THE VOWEL UM (HUM) WAS REPORTED IN 28 PER CENT OF THE 300 TRIALS

Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	28	18	17	11	5	7	3	2	3	2
iu (cure)	16	15	18	11	9	21	14	13	10	7
au (now)	0	3	3	0	1	0	0	0	0	0
ō (fold)	10	10	11	8	10	16	5	4	2	0
o (not)	1	2	1	2	2	0	2	1	0	0
ä (far)	29	22	21	15	11	6	11	5	5	4
a (fat)	1	1	4	6	10	3	3	3	3	3
e (set)	0	1	1	5	7	6	4	5	6	4
ē (they)	2	4	0	10	4	8	5	7	5	6
i (sit)	2	2	1	4	5	8	20	20	17	13
ī (see)	1	2	6	3	7	7	11	20	30	32
ai (fine)	0	2	2	1	8	4	7	8	4	11
failed	10	18	15	24	21	14	15	12	15	18

Average failures = 16 per cent.

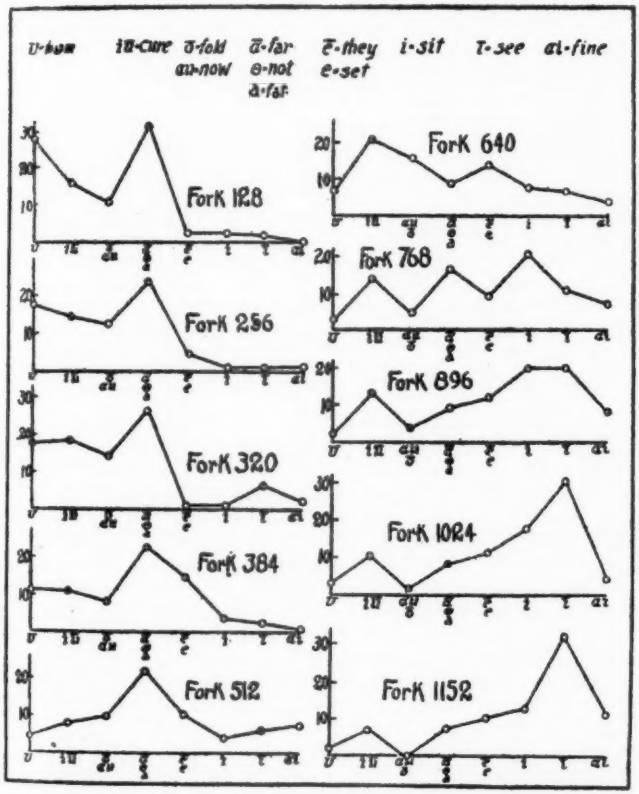
Table III. Perhaps the characteristic thing in the data of the teachers was the greater variety of vowels used, twelve as compared with eight for the Series A of the elementary students. The short vowels o (not), a (fat), e (set), i (sit), were used much more frequently than in the preceding experiments. The frequency of these short vowels is considerably less than of the long vowels, but there is clear evidence that the influence of the duration of the sound on the character of the vowel has been discriminated. This would be expected since the quantity of vowels is one of the important factors in teaching a non-hearing student to speak.

The relatively large percentage of failures was due partly to the instructions not to record the instances in which it was

impossible to decide upon the vowels, and partly to the novelty of being subjects in an experiment. The university students were more familiar with class demonstrations and laboratory work and the impressiveness of the occasion did not sit so

CURVES III

TEACHERS OF DEAF CHILDREN—CLASS EXPERIMENT—THIRTY SUBJECTS—VALUES TAKEN FROM TABLE III—EACH FORK PLOTTED SEPARATELY —VOWELS WHICH HAVE BEEN COMBINED ARE SHOWN UNDER EACH OTHER



heavily on their shoulders. Furthermore the attitude of 'trying to make a high score' was not so successfully elimiuated with the teachers and this always has an inhibiting effect on the reactions of more mature individuals.

Curves III. To facilitate comparison with Experimeuts I and II the short vowels have been grouped with the corresponding long ones. The shapes of the curves correspond in general with the curves of Experiment I. The high frequency for ä (far) for the first five forks, seemed anomalous at first, but some of the teachers pointed out that this was the most ambiguous vowel of the series and where the discrimination was difficult this vowel served the purpose of a general utility vowel better than any of the others.

Considered as a whole the data may be regarded as complementary to the other experiments. It must be remembered that these teachers had not been trained for this particular type of discrimination. Their theories of vocalization, methods of teaching oral speech and past speech habits influenced their reactions to the auditory percept of the fork tones. Considering these factors it is rather remarkable that the results are as uniform as we find them. If these teachers had been asked to discriminate the series of fork tones into twelve degrees of quality, brightness or volume, with as little preliminary instruction as they received in discriminating the vowel character, the experiment probably would have failed.

IV. The Distribution of Vowels

To determine the differences between the vowel groups, the results of all three experiments were combined so as to include all the vowels given in Tables I, II, III. Such a combination will approach the average ability in vocality discrimination that would be expected from those subjects who are likely either as students or as teachers to exhibit a detailed interest in audition because of its practical or esthetic significance and to whose needs auditory terminology should be adapted if there are no scientific limitations Thus if the term vocality is adopted it should not be difficult to explain to a musician what is meant by the ü-character of the lower end of the musical scale or the ï-character of the upper end.

No attempt is made to allow for differences in training, age, sex, experimental method and the variable number of series. Weighting separate experiments no matter how cleverly done, can never be a substitute for a uniform method in a single experimental plan. Yet since the aim is qualitative and the work

is regarded as preliminary to future intensive work, such a combination will give a conservative sketch that will help in developing a fruitful methodology.

TABLE IV

PERCENTAGE OF VOWELS ASSIGNED TO EACH FORK WHEN DISCRIMINATIONS IN TABLES I, II, III ARE COMBINED

Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
um (hum)	26	14	11	7	2	3	1	1	1	1	7
iu (cure)	44	25	25	17	12	16	14	10	8	5	17
au (now)	0	1	1	0	0	0	0	0	0	0	0
ō (fold)	12	21	19	21	15	16	8	4	3	2	12
oi (boy)	0	1	2	2	3	2	4	2	1	2	2
o (not)	0	0	0	1	0	0	1	0	0	0	0
ä (far)	13	17	21	23	13	8	11	5	5	4	12
a (fat)	0	0	2	3	4	1	2	2	1	1	2
ē (they)	2	14	9	12	16	18	19	20	10	10	13
e (set)	0	0	0	2	3	2	2	2	3	1	2
i (sit)	0	0	0	2	3	4	12	11	10	9	5
ai (fine)	0	2	3	4	14	12	12	18	24	27	12
I (see)	3	5	7	6	15	18	14	25	34	38	16
Total...	750	726	732	698	705	721	584	730	729	709	

Total includes only the actual judgments. Percentages are derived from actual judgments made, failures not included.

TABLE IVa

DATA OF TABLE IV GROUPED INTO VOWEL CLASSES

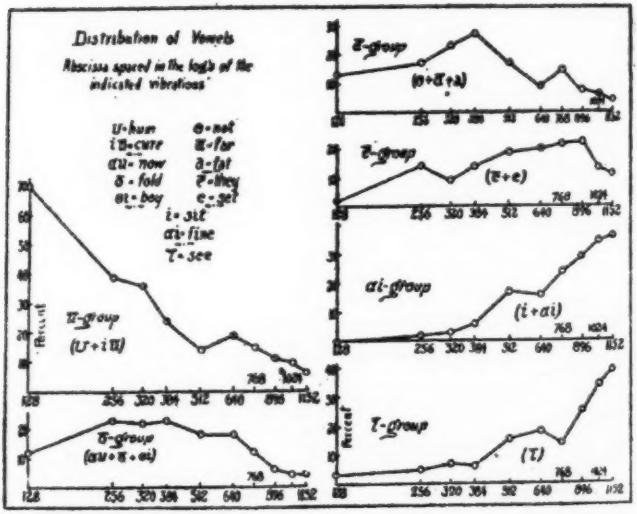
Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
ū-group	70	39	36	24	14	19	15	11	9	6	24
ō-group	12	23	22	23	18	18	12	6	4	4	14
ä-group	13	17	23	27	17	9	14	7	6	4	14
ē-group	2	14	9	14	19	20	21	22	13	11	15
ai-group	0	2	3	6	17	16	24	29	34	37	17
i-group	3	5	7	6	15	18	14	25	34	38	16

Tables IV and IVa. All the vowels that were assigned in any of the experiments are given in Table IV. The last line marked 'Total' indicates the actual number of judgments only. If the failures had been included the total for each fork would have been 800. The low total of 584 for fork 768 is due to the fact that this fork was not available for Series B of

Experiment I. The last column (Rel. Fr.) gives the relative frequency of the vowels in per cent. Only six vowels have a frequency greater than ten per cent and these are selected as the more important classes. In Table IVa those vowels having a frequency of less than ten per cent are grouped together with those of the six important vowels to which they seem to belong. This gives the six *groups* of vowels shown in the first column. Each of these groups has a frequency that is large enough to determine the approximate vibration rate where the maximum frequency for a given vowel group lies.

CURVES IV

COMBINATION OF EXPERIMENTS I, II, III. SHOWING FREQUENCIES OF THE VOWEL GROUPS AT THE DIFFERENT VIBRATION RATES AS TAKEN FROM TABLE IVA.



Curves IV. The curve of the ū-group shows its maximum very decidedly at the fork 128. For the ō-group no decided maximum appears although its position is probably between 256 and 384 vibrations. The ā-group has a decided maximum at 384 and the drop on either side seems to justify the assumption that its highest frequency is more toward the higher forks than the ō-group curve. The ē-group curve is somewhat am-

biguous but in the reverse sense of the ō-group, its maximum seeming to lie between 512 and 896. The ai-group curve is similar to the ī-group; the maximum is at the highest fork used in the series, but shows evidence of having reached its highest frequency. For the ī-group the maximum is also with the highest fork but the slope of the curve at this work indicates that the point of highest frequency has not been reached.

A general consideration of the curves substantiates the statement made by Titchener (8) that the lower tones have a definite ū-character and the higher tones a definite ī-character.

V. The Consistency Between Vowels

The consistency in making the discriminations in these experiments approaches two limits:

1. When the consistency is least a subject will assign any vowel taken at random from his repertory of vowels every time he is asked to report the vowel character of any tone.

2. When the consistency is greatest a subject will assign the same vowel to the same fork every time it is presented.

The consistency may be measured by squaring the number of times a given vowel is assigned to a given fork. Thus if out of ten trials with fork 128 a subject reports 'u' *five* times, 'o' *two* times, and 'um' *three* times, the sum of the squares of the frequencies (5, 2, 3) is $25 + 4 + 9 = 38$. The number 38 measures the consistency.

If the subject had reported only a *single* vowel for all ten times, this would give a frequency of 10 for one vowel and the sum of the squares (since there is only one vowel) is $10^2 = 100$. The value 100 represents the upper limit or maximum degree of consistency.

If the subject assigns ten *different* vowels, each vowel can occur only once and the sum of the squares is $10 \times 1^2 = 10$. This represents the lower limit or minimum degree of consistency.

To make the measure of the consistency independent of the number of trials, the sum of the frequencies of the vowels squared, is divided by the square of the number of trials. By doing this the maximum consistency becomes 1.00 no matter how many trials are given, but the minimum will depend upon the number of trials.[6] Since the vowels available are relatively

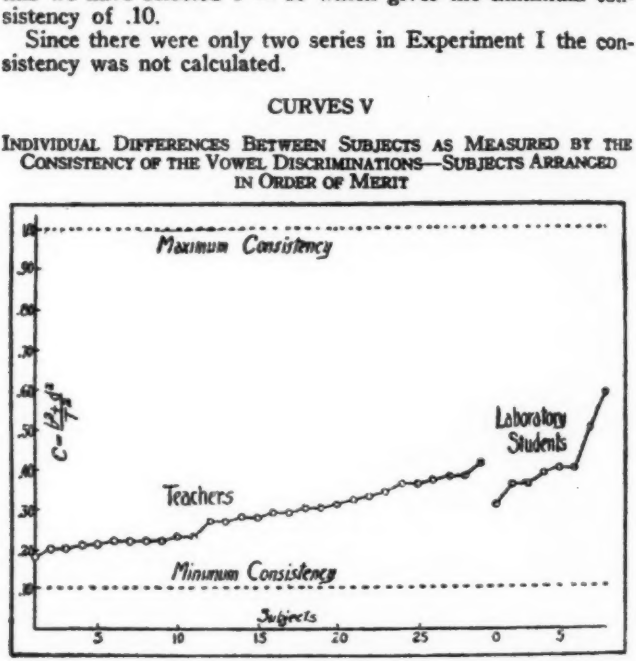
[6] The consistency (C) is algebraically expressed as follows: Let T represent the number of times a given fork is presented, and $b + d$ the number of times each vowel is assigned. Then $C = (b^2 + d^2) / T^2$.

few and both groups used the same vowels we can regard the minimum consistency as equal for both groups ot subjects. For this we have selected $T = 10$ which gives the minimum consistency of .10.

Since there were only two series in Experiment I the consistency was not calculated.

CURVES V

INDIVIDUAL DIFFERENCES BETWEEN SUBJECTS AS MEASURED BY THE CONSISTENCY OF THE VOWEL DISCRIMINATIONS—SUBJECTS ARRANGED IN ORDER OF MERIT



Curves V. The dotted horizontal lines indicate the maximum and minimum limits of consistency. Each small circle on the intermediate curves represents one subject, and these have been arranged according to their degree of consistency. The vertical coordinate represents the average consistency for all the forks.

Considering the curve for the teachers, we find it relatively flat and that it does not indicate great individual variations. The median is at about 28 and this means that on the average

If the same vowel is assigned each time $(b^2 + d^2) = T^2$ and $C = T^2 / T^2 = 1.00$, the *maximum consistency.*

If a different vowel is assigned each time $(b + d) = T$ or $C = T / T^2$ the *minimum consistency.*

each subject assigned about three different vowels to each fork. For the laboratory students the median is at 40 indicating that on the average two different vowels were assigned to each fork. Out of a possible thirteen vowels actually used this represents a fair degree of reliability in making vocality judgments. If only the vowel *groups* indicated in Curves IV had been used in the measurement of the consistency it would have been more than twice as great and the curves would lie about half-way between the maximum and minimum limits.

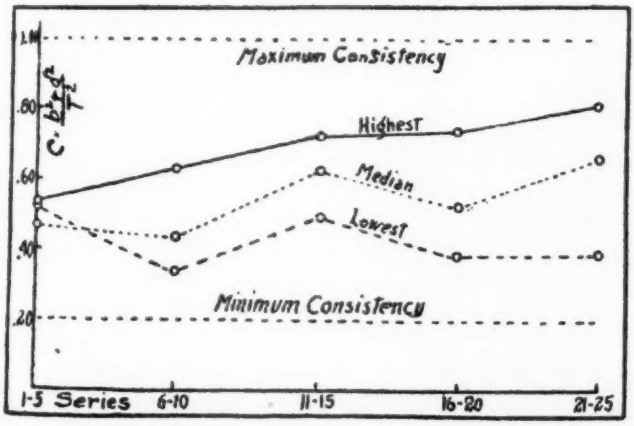
The individual differences between subjects conform to the usual results of those psychological experiments in which there is a gradual transition from one degree into another as for instance in auditory acuity. There is no evidence of a tendency toward a separation into two classes as into musical and non-musical subjects.

VI. Effect of Practice on the Consistency

In the Series B of Experiment I the consistency is much greater than for the Series A. The question arises what is the effect of practice upon the variability of the discriminations? In Curves VI the consistency for three subjects from

CURVES VI

Practice Effect in Vowel Discrimination—Three Subjects From the Laboratory Class—The Consistency is Calculated After Every Five Series



Experiment II was calculated for successive groups of five series each. The curve marked 'highest' represents the subject whose average consistency for the twenty-five series was greatest. The 'median' curve represents the subject who stood intermediate, and the 'lowest' curve represents the subject whose discriminations varied the most.

The curve for the 'highest' subject shows a gradual increase in the uniformity with each succeeding group of five series. The rise is not marked and signifies that a high degree of consistency had already been reached after the fifth series. For the 'median' subject there is a slight oscillation which probably implies that the consistency is no longer a function of training or practice after the first five series. This is more clearly shown by the 'lowest' subject whose curve shows even a slight drop between the fifth and twenty-fifth series.

The curves seem to indicate that the ability to discriminate vowels in tuning fork tones reaches a constant level after less than five series or after fifty discriminations. This of course does not mean that the maximum acuity has been attained. It merely indicates that the subject has isolated the vocality factor and that he is not confusing it with pitch, intensity, etc.

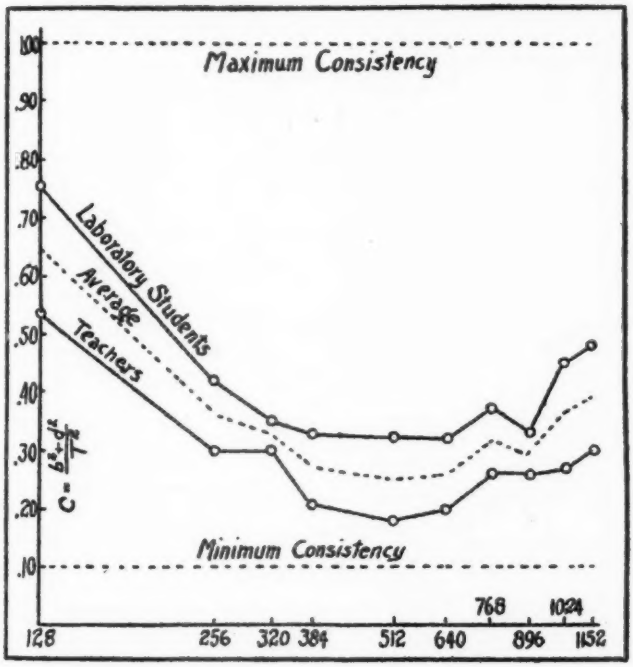
VII. The Consistency Between Vocality and Vibration Rates

The preceding topic indicated the individual differences between subjects. In Curves VII the individual differences of the forks, so to speak, are shown. The nearer a point on the curve approaches the dotted line indicating the minimum consistency the greater is the number of vowels assigned to it. To show the octave intervals as equal, the base line has been *spaced* in the logarithms of the vibration rates, although for easy reading the absolute vibration rates are given.

The curves clearly reveal that the forks in the middle region are more variable than the extremes. This is the opposite of pitch relationship or tonality which shows finer discriminations in the middle regions. The average consistency for the forks is indicated by the dotted curve. In consideration of the fact that these curves are based upon thirteen vowels, the degree of consistency is rather high. If only the vowel *groups* had been taken the curves would have been shifted upward to a median position between the limiting degrees of consistency and this would imply that on the average not more than two vowel groups would be reported for any one fork.

CURVES VII

RELATION BETWEEN VIBRATION RATE AND NUMBER OF VOWELS DISCRIMINATED—SUBJECTS FROM EXPERIMENTS II AND III



VIII. COMPARISON WITH THE WORK OF KOEHLER

In comparing the results of our experiments with those secured by Koehler it is necessary to allow for the different conditions under which the data was secured. Our comparison curve is a combination of three separate experiments. Koehler's curve is the result of theoretical deductions drawn from his own experiments and various other sources. Along the base of the Curves VIII the vibration rates of the forks are shown, spaced in logarithms.

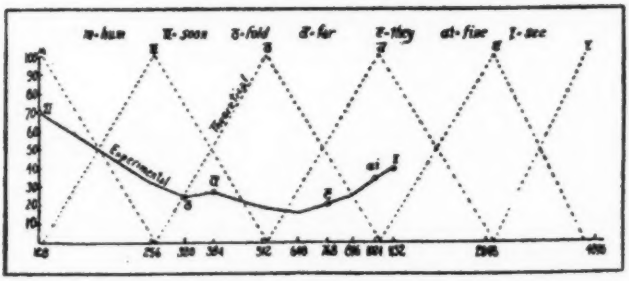
The dotted oblique lines represent Koehler's theoretical assumptions on the principle that each vowel has a maximum

frequency at some absolute vibration rate, with the frequency receding on either side until at the adjacent octaves its frequency is zero. These adjacent octaves however are the maxima for other vowels. Thus Koehler gives the vowel ū (soon) at 265; that is the vowel character of a pure tone of 265 vibrations is ū (soon) and under ideal conditions this vibration rate would be characterized as ū (soon) in 100 per cent of the judgments. As the vibration rate is increased, the subjects would characterize the tone as ū with a trace of ō. A further increase in the vibration rate would make the ō component stronger until a rate is reached at which the vocality resembles ū and ō. If at this vibration rate, which is indicated by the points at which the oblique lines cross, a number of subjects are required to report which of the two vowels ū or ō the tone most resembles, half of the subjects would report ū and half would report ō.

As the vibration rates are increased from this point the ò component becomes stronger until at 526 there would be 100 per cent ō judgments. In the same manner, as the vibration rate is decreased from 265 the vowel character approaches um (hum) until at 132 we have 100 per cent um judgments but none of ū. Whether or not the maxima should be connected with the respective minima by straight lines, Koehler leaves open for the present.

CURVES VIII

COMPARISON OF THE EXPERIMENTAL RESULTS WITH KOEHLER'S THEORETICAL ASSUMPTIONS—BASE LINE IS SPACED IN THE LOGARITHMS OF THE VIBRATION RATES



In order to combine the results of our experiments into a single curve upon which all the vowels are represented, rather than drawing a confusing nest of curves showing each of the

forks separately, we have retained only the maximal points of the separate vowels. For the intermediate forks we have taken from Table IVa the average of both the percentages involved. For instance, the maximum for the ū-group is at fork 128 and the per cent is 70; the similar fork for the ō-group is 320 and the per cent is 22; at the intermediate vibration 256 the per cent for the ū-group is 39 and for the ō-group 23. The average of 23 and 39 is 31 which is the value we have selected for our experimental curve at 256. For all the other forks, which were not themselves the maximum for a vowel group, the same method was adopted.

Whenever untrained subjects or general class experiments are used, there is usually a masking of those effects that would be revealed by more intensive methods. This masking may be so complete as to be a virtual obliteration. If however there are *relative* maxima and minima shown in the graphical representation of the qualitative results these should coincide more or less closely with the quantitative experiments just as the relative maxima and minima of the quantitative experiments should coincide with the theoretical limits of ideal conditions if the theory represents the facts.

From the experimental curve it will be seen immediately that there is only one decided maximum at ū (soon) and a less distinct one at ī (see). Our ū coincides with Koehler's *m*, but none of the other vowels correspond with Koehler's nor is there any evidence of symmetry in their distribution. By shifting our curve to the right one octave all of our vowels are brought within Koehler's octaves. But whether this rather strenuous stretching is justified by the difference in language habits (English as compared with German) will remain doubtful until this factor is itself made the basis of a special study.

IX. Discussion of Vocality

Relation between Vocality and Vowels. Perhaps the striking thing in making vocality discriminations is the fact that the fork tones do *not* sound like the vowels used in language but yet may be said to resemble them so closely that consistent judgments are soon made upon this basis.

Meyer regards the vocality as a continuous series limited on the one hand by the degree of mellowness of the lowest tones and on the other by the degree of shrillness of the highest tones; following this conception we may regard the vocality of the intermediate vibrations as varying combina-

tions of mellowness and shrillness. However to ask a subject to differentiate *degrees* of mellowness and shrillness directly, does not result in consistent reactions nearly so rapidly as when he is asked to discriminate the vowel character. Obviously this can only mean that the subject is not so familiar with the terms mellowness and shrillness as he is with the vowels of language.

Conventionalized Vocality Combinations. The vowels may be regarded as special names that have been given to those combinations of mellowness and shrillness that have been conventionalized into language elements. The vowel in actual speech is an auditory *complex* and its particular combination of mellowness and shrillness is only one of the components—that component however from which the vowel gets its name. There are of course many more combinations of mellowness and shrillness than manifest themselves in the vowels of any one language, but in speech an infinite number of vowels would be confusing and unnecessary. A dozen vowels meet the practical requirements of any one language. Convention and usage have determined which of the combinations shall be selected and the use of these has developed a degree of facility in their discrimination which gives them a predominance over all other possible combinations of mellowness and shrillness that are not used in language. The relative distinctness between vowels is thus merely an expression of the fact that we have selected and conventionalized those combinations which were most easily discriminated.

If the experimental conditions are such that the subject is asked to discriminate combinations of mellowness and shrillness that have not become conventionalized through their being a component of the language vowels, he describes them by the nearest of the conventionalized combinations (one of the vowels in his particular language) to which he already has learned to react. We might expect further that the vowels are rather closely related to absolute vibration rates because on the whole the range of the *average* human voice is rather restricted.

The Intervals in Vocality. The octave relationship which seems to exist between vowels may be regarded as a manifestation of the Weber-Fechner law. If a continuous physical series such as the frequencies of sound waves is to be divided into definite sensorial steps such as the vowels, the division will occur in equal logarithmic intervals rather than in equal absolute vibration intervals because the logarithmic intervals will give sensorial steps in which the dissimilarity between

adjacent steps is equal and greatest. That is, the equal logarithmic steps would become conventionalized rather than would the equal absolute vibration steps. This of course, would give a kind of an octave relationship for the given language. In other languages the apparent octave relationship may be based upon entirely different combinations of mellowness and shrillness. From the vowels and semi-vowels of all languages we might be able to construct a practically continuous series of all possible combinations of mellowness and shrillness.

Definition of Vocality. In the interest of uniformity in terminology the following definition or description of vocality is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themselves as a resemblance to the vowel and semi-vowel sound: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H [pronounced as the *ch* in *loch* (Scotch) or mi*ch* (German)] is the shrillest.

This definition or description is eclectic and is derived from suggestions of Stumpf, Meyer, Koehler and Titchener. If all investigators could agree to call this one property *vocality* and nothing else, and if they would exercise some restraint in the coining of new terms until they had assured themselves that they were describing a form of discrimination that had not been made by others, greater uniformity in terminology would result. If for instance, it is impossible to discriminate between two attributes of sensation then we have only one attribute and there is no advantage in giving this attribute two or more different names. If brightness (*Helligkeit*) cannot be discriminated from shrillness, then there is no difference between brightness and shrillness and one of the terms is sufficient. Which of the two terms will be adopted is of course immaterial. Priority in use is the simplest criterion.

X Summary and Conclusions

The problem was to determine,

A. How consistently relatively untrained subjects could discriminate the vocality or vowel character of tuning forks ranging from 128 to 1152 vibrations.

B. Whether the term vocality or vowel character is a suitable one to describe an attributive or cognitive fact of audition.

A. Consistency of Vocality Discrimination

From experiments with the elementary students (I)[6] we find even with only a single series that the ū (soon) character is predominant for the lower forks and the ī (see) character is predominant for the higher forks. The intermediate vowel classes, ō (fold). ā (far), ē (they), do not seem to have dominant positions at any particular vibration rate; but there is no question that they occur as a series without the possibility of interchanging their relative positions.

The results from the experiments with the laboratory students (II) show the same thing but in a more orderly manner The discriminations of the teachers for the deaf (III) approach the results of the more specifically trained laboratory students (II) as well as might be expected when we consider the larger number of vowels used.

When the findings of all the experiments are combined (IV) and the vowels are reduced to six groups, each group has a relative frequency (Table IVa) which makes it probable that the group of vowels are associated in an orderly manner with corresponding vibration rates, but our experiments do not exhibit an octave or interval relationship (VIII).

There is no evidence of marked individual variation (V) between subjects in their general ability to discriminate vocality such as is supposed to exist between musical and non-musical subjects. A high degree of consistency is reached after relatively few vowel discriminations. For the elementary students (I) a marked improvement is shown after only one series. For the laboratory students (VII) the degree of consistency at the end of the fifth series (a total of fifty single discriminations) has practically reached the limit that is reached after 250 discriminations. The consistency of the vowel discriminations for the different forks (VI) is highest for the extremes and lowest for the intermediate forks.

B. The Term Vocality

From the rapidity with which the discriminations of vowel character become consistent (I, VI) it seems evident that when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well estab-

[6] The roman numerals in parentheses refer to the headings from which the conclusions are derived.

lished by the ordinary (speech) habits of the individual. Practice (I, VII) improves the vocality discriminations so rapidly that no extended demonstrations are necessary to bring out the meaning of the term.

Improvement in the discrimination occurs spontaneously (III) and manifests itself by the addition to the vocality series of those vowels which are formed by lengthening or shortening the vowel sounds. Even when the discriminations become consistent the subjects (II) still report that while two tones of different vibration rates may be classed as resembling the same vowel there is still a difference between them which can be described as an admixture of another vowel sound but the degree of admixture cannot be reported with a high degree of consistency.

As a definition or description of the term vocality (IX) the following is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themslves as a resemblance to the vowel and semi-vowel sounds: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H, pronounced as the *ch* in lo*ch* (Scotch) or mi*ch* (German) is the shrillest.

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THE VOCALITY OF FORK, VIOLIN AND PIANO TONES

By Esther L. Gatewood, Carnegie Institute of Technology[1]

Problem—The immediate purpose of this study has been to investigate the validity of the theory of absolute vocality as it has been set forth by Köhler and others, when applied to tuning forks, piano and violin. If the assumptions as made be true, then we may expect to find a *relative* degree of consistency between the several instruments. A tone of a given pitch should according to the theory have the same vocality, whatever the instrument that produces it, provided the vocality of the over-tones is considered. Köhler[2] assumes first of all that vowels belong to definite frequencies. Willis and other investigators also found that vowel character is dependent on frequency. Köhler further assumes that all vowel characters may be defined in terms of the proportion of ū, ō, and ī quality. In other words, these three are most definitely located with ū at the lower end of the scale, ī at the upper and the pure ō sound midway between. As one leaves either end of the scale, the vowel quality characterizing that end becomes less and less prominent and that of the other end increases. Those vowel sounds which his observers definitely located, m, ū, ō, ä, and ē. were of such frequencies that their pitches represent successive octaves in the order named.

Observers—The observers were children from a first grade intermediate school, i. e., from the 7B to the 9A grades inclusive. The ages of the children ranged from 11 to 19, in numbers as follows:

No. of Children	*Age*
11	11
34	12
52	13
54	14
36	15
7	16
2	17
0	18
1	19
2	?

This made a total of 199 children with an average age of 13.3 and a median age of 13. Of these children, 123 had studied some little music outside of that regularly given in the public schools, and 76 had

[1] This research was done, under the direction of Dr. Weiss, at the Ohio State University.

[2] Köhler, Wolfgang, 'Akustische Untersuchungen, II,' Zeitschrift für Psychologie, 1911. 58, pp. 59-140.

not. No definite information could be obtained as to the amounts, so it was merely recorded as 'some' or 'none.' As a general rule, it meant a few lessons on the piano. In addition to the 199 children to whom the three series, i. e., forks, piano and violin, were given, 33 others were given the fork series and the violin series and 26 the fork series only. These results however are not included in the data herein given, as it was desired to have results from the same children for all three series.

Material—The material used consisted of a series of tuning forks including the frequencies 256, 512, 1024, 1280, a piano, and a violin. The experimenter was a performer on both these instruments and was thus able to handle them advantageously for such an experiment.

Method—The experiments were performed in a room in which there were tables and chairs across either end. Approximately twenty children were given the experiments at one time. They were seated at the tables in such a manner that their backs were to the experimenter who played the instruments. The position of each child was about ten feet from the source of the sound.

After the forks had been shown to the children and the method of obtaining a sound from them had been explained, the following instructions were given:

Listen carefully to each fork as it is played and then when I have finished with it, write down opposite the number (serial number, as 1. 2. 3., etc.) the word or vowel it sounds most like to you. I shall play each tone three times. Listen carefully till I have finished and then write down the word. If you can not think of one that exactly fits, put the best thing you can think of; if you can think of none, leave that space blank.

After it was certain that the children understood what they were to do, the first series was presented, giving 256, 512, 1024, 1280, and then a repetition of the same with the order reversed. The same tones were then given in the piano and violin series with the addition of 384 preceding and 1536 following the series as given above. An interval of two or three seconds was allowed after the one tone rung off before it was repeated. Between tones of different pitch sufficient time was allowed to have each child note down the word or sound that he thought represented the tone just given.

The work was marked by interest and splendid cooperation on the part of the children. From the whole number tested, (more than 200) only a very few records had to be discarded because they showed lack of effort or a misunderstanding of instructions on the part of the observers. These were for the most part papers in which the children had tried to give the tonic-sol-fa equivalent for the tone given, which response is one of pitch rather than of vocality.

Results—Obviously in so large a number of records, a great variety of words will appear. The number of actual vowel sounds represented however is much smaller. For example, the words, moon, too, threw, blue and many others all represent the vowel sound ü. Likewise, the words, eat, me, seat, fiend and many others represent the sound ī. Among children we do not find the variety of vowel sounds that we do among adults. This is probably due to the narrower limit of the vocabulary of the child as compared with the adult, as well as the lack of development in finer discriminations of sound. It is often noted that children do not make the fine distinctions in the pronuncia-

tion of words that we find in the average adult. In this instance the sounds which they gave, fall rather naturally into twelve or thirteen groups. In the tables thirteen are given, but the numbers for one group, namely that with the sound short ŏŏ as in took, are very small. These vowel sounds and the number of observers giving such sound for each tone, are recorded in Table I.

TABLE I

NUMBER OF OBSERVERS, VOWEL CLASSES AND VIBRATION RATES FOR FORK, PIANO AND VIOLIN TONES

Fork	ū who	ō know	ă saw	au how	u hum	a fat	ä far	ē prey	i hit	aī pine	e get	ī feel	ŏŏ took
256	123	17	15	0	32	2	0	1	1	1	0	1	0
512	86	30	10	7	13	2	0	4	1	1	7	19	3
1024	38	45	4	9	6	1	2	10	10	3	13	45	0
1280	24	16	5	8	9	1	5	3	24	13	13	52	2
1280	17	21	8	8	10	2	4	4	23	18	7	59	0
1024	40	47	13	14	11	0	3	5	9	9	9	19	0
512	74	48	3	10	13	2	2	5	7	2	8	5	1
256	93	26	8	9	23	0	2	4	2	2	7	2	2
Piano													
384	49	56	4	5	13	3	3	4	3	1	3	8	1
256	62	61	5	4	14	2	0	3	0	5	4	4	0
512	15	18	7	4	17	2	6	11	11	11	7	44	0
1024	17	18	4	6	8	2	6	4	12	13	7	53	0
1280	7	18	4	2	8	2	2	7	18	18	10	54	0
1280	6	16	6	1	6	1	2	5	15	14	13	60	0
1024	33	29	8	6	12	4	3	6	8	12	17	11	1
512	29	37	13	4	22	4	4	11	1	4	8	6	1
256	39	59	11	3	17	0	2	2	2	7	7	2	0
1536	8	18	3	3	7	2	2	3	24	6	39	30	0
Violin													
384	23	44	13	6	27	1	3	10	4	5	15	14	7
256	47	57	12	7	21	2	2	11	4	1	2	9	1
512	15	28	3	3	9	3	2	9	7	13	20	61	0
1024	5	7	2	4	6	2	1	8	13	16	14	93	0
1280	10	9	2	5	11	0	3	5	22	20	6	78	0
1280	6	10	4	1	5	3	4	5	17	11	8	100	0
1024	16	15	7	8	11	0	2	13	19	9	14	49	3
512	23	48	15	5	16	1	3	19	8	4	12	14	6
256	33	60	14	11	17	3	6	7	1	12	5	1	4
1536	10	11	3	0	4	1	1	12	20	7	32	63	2

The numbers in the first column indicate the vibration rate of the tone used in each case. The numbers in the remaining columns indicate the number of times the vowel sounds at the top of the columns were given as corresponding with the tones indicated at the left.

For example, when tone 256 was sounded, from the forks, 123 observers reported the vowel sound u, 17 observers the vowel sound o etc. These figures are represented graphically in Figure I.

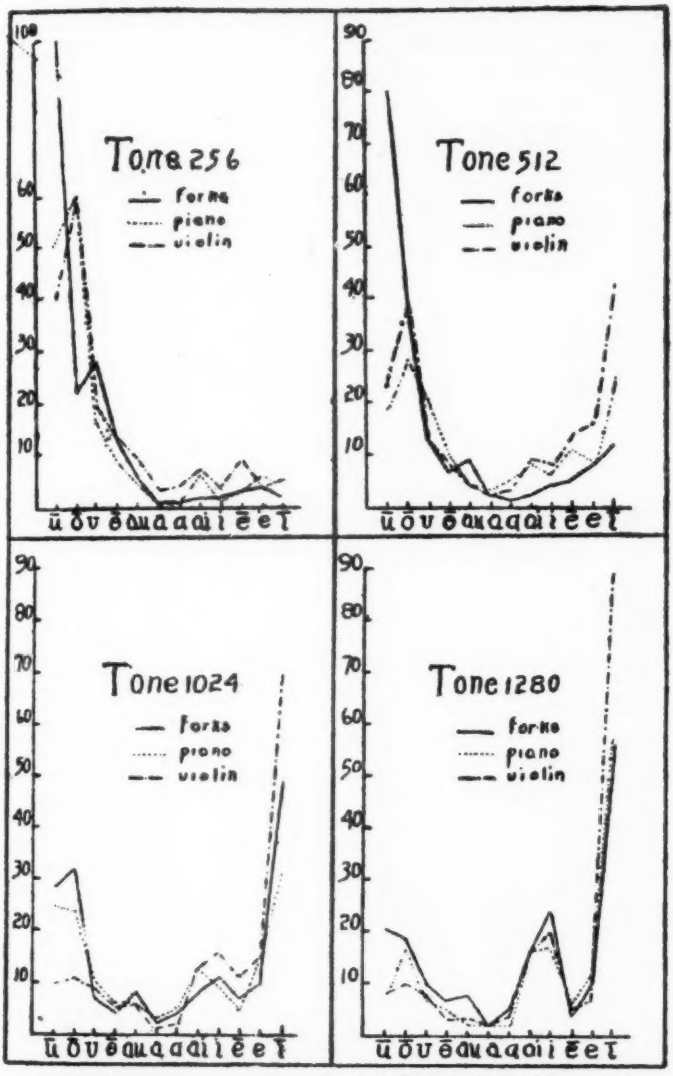

Fig. I—Graphical Representation of Results in Table I. Both trials averaged.

TABLE II

VIBRATION RATE AT WHICH VOWEL WAS MOST FREQUENTLY RECORDED

Vower	Forks	Piano	Violin	Identity
ū (who)	256	256	256	3
ō (know)	1024	256	256	2
ă (saw)	256	512	256	2
au (how)	1024	1024	256	2
u (hum)	256	512	256	2
a (fat)	512	512 (1024)	256	2
ä (far)	1280	512	256 (1280)	0
ē (prey)	1024	512	512	2
i (hit)	1280	1280	1280	3
ai (pine)	1280	1280	1280	3
e (get)	1024	1024	512 (1024)	3
ī (feel)	1280	1280	1280	3
ŏŏ (took)	512	512 (1024)	512	3

The table reveals that eight of the vowel sounds were placed at the same tone for forks and piano; for eight the forks and violin agree. In six all three agree. Noting the number of times that any particular vowel sound was found throughout the whole series, we find that the numbers for ä, ă, and ŏŏ (Table I) are very small as compared with the other vowel sounds. These vowels might have been combined with other vowels but it was thought best to give actual results without any arbitrary attempt at grouping.

A very large majority of the observers agreed that the sound ū characterizes the tones of the lower end of the scale, and that the sound ī characterizes the higher. Between these limits there is more variation and diversity of opinion, than at the extremes. This fact is shown in Table III, in which is recorded the vowel sound which was given by the largest number of observers for a given tone.

TABLE III

VOWEL SOUND REPORTED MOST FREQUENTLY FOR EACH TONE

Tone	Forks		Piano		Violin	
	I	II	I	II	I	II
256	ū	ū	ū	ō	ō	ō
512	ū	ū	ī	ō	ī	ō
1024	ō and ī	ō	ī	ū (ō)	ī	ī
1280	ī	ī	ī	ī	ī	ī
384			ō		ō	
1536			ē		ī	

I and II refer to first and second trials. For fork 1024, vowels ō and ī were given by the same number of observers (45 each). For tone 1024 when sounded from the piano, ū was given by 33 observers and ō by 29.

It was thought desirable to calculate the consistency[3] or reliability of the vowels assigned to the various vibration rates. The formula $C = (b^2 + d^2)/T^2$ was used. For example, if tone 256 is sounded ten times, and the vowel *ü* is reported three times, the vowel *ō* four times, and the vowel *au* three times, an application of the formula gives $C = (3^2 + 4^2 + 3^2)/10^2$ or reduced, $C = 34$. If the same vowel is given each time, we get $10^2/10^2$, a maximum consistency of $C = 1.00$. if a different vowel is recorded each time or by each observer we get 1^2.....10 times or $10/10^2$, a minimum of $C = .10$. In Table IV are recorded the consistencies for each tone and for each instrument.

TABLE IV

CONSISTENCY FOR EACH VIBRATION RATE

	256	512	1024	1280
Fork	.34	.23	.14	.13
Piano	.17	.08	.09	.11
Violin	.15	.11	.17	.23

THESE FIGURES REPRESENT THE AVERAGE OF THE CONSISTENCIES AS CALCULATED FOR THE FIRST AND SECOND TRIALS—FIG. II SHOWS THE COMPARISONS MORE CLEARLY

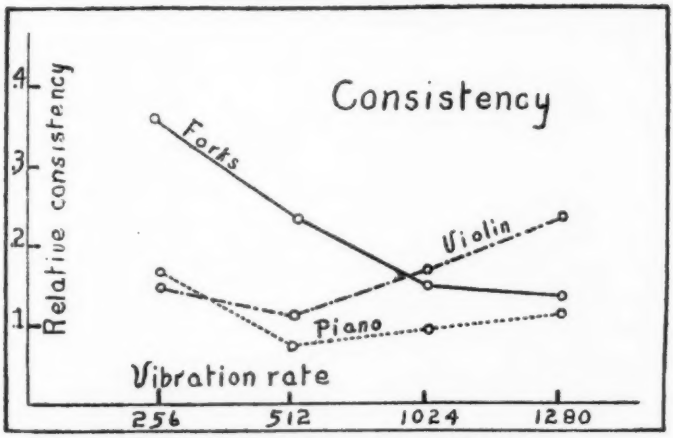

Fig. II—Graphical Representation of Table IV.

Among 200 observers, with all the possibilities of vowel sounds before them, it is not surprising that we do not get a greater con-

[3] In conformity with a similar article by A. P. Weiss it was thought best to adopt this formula.

sistency. We find that the curves of consistency for the forks and the piano are approximately the same shape, but in the curve representing the tones as given by the violin, there is a rise at the upper end, instead of the lower. Miller[4] has found that the lower tones when sounded from the violin have weak fundamentals. Furthermore, the resonance of the body of the violin is such that it emphasizes the higher tones and their partials. The lowest tones on the violin are lower than the fundamental resonance of the violin body. On the average, the consistency for the forks is greatest, the violin second, and the piano least.[5] The consistency or agreement between the two trials is also greatest for the forks, for the violin second, and for the piano least (Table III).

Another interesting result is that gained by comparing the vowels given for the two lowest tones and those for the two highest tones of the series Fifty-three per cent of the observers gave either ū or ō as the vowel representing the vocality of the tone 384 as sounded by the piano, whereas 62 per cent gave these vowels for the vocality of tone 256. This fact shows a discrimination of the lower vowel quality of the tone 256 as compared with tone 384. Likewise, 37 per cent of the observers gave the five vowels representing the highest vowel quality,[6] when tone 1024 was sounded, whereas 52 per cent of the observers gave these high vowels when tone 1280 was sounded. This shows a discrimination of the ī vowel quality as the upper limit of the scale is approached.

In the violin series, 59 per cent of the observers gave the vowels of the lower end of the series after tone 384 was sounded, whereas 74 per cent gave these same vowels to the tone 256. This shows a discrimination of the lower vowel quality of the tone 256. Likewise, at the upper end of the scale, 50 per cent of the observers gave the ī, āi, or ī vowel sounds to the tone 1024, whereas 62 per cent gave these vowels to the tone 1280. This shows a discrimination of the increase of the ī quality as the upper limit of the scale is approached.

The addition of the two tones to the piano series and the violin series furnished a test of the children's ability to recognize the change in vowel character, when the change was not a part of the progressive series. A study was made of each individual's response to the change, i. e., whether he changed from a lower vowel sound to a higher, kept the same vowel for both tones, or changed from a higher to a lower vowel sound. These results are given in Table V.

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[4] Miller, Dayton, Clarence—The Science of Musical Sounds, 1916, p. 197.

[5] The question as to whether the consistency of different instruments changes for different vibration rates is an interesting problem, but one for which this data is insufficient.

[6] These figures were obtained by adding the number of au, e, ī, i, and ai vowels given for the tone 1024 (or 1280 as the case may be). These vowels are given the greatest number of times at either 1024 or 1280, when sounded from the piano. (Table II).

TABLE V

VOWEL CHANGES RECORDED WHEN TONE CHANGE WAS NOT A PROGRESSIVE ONE

Change	Lower to Higher	Same for Each	Higher to Lower
Piano			
From 256 to 1536	66%	18%	16%
From 384 to 256	18%	61%	21%
Violin			
From 256 to 1536	72%	21%	7%
From 384 to 256	19%	35%	46%

The results show that when the change in interval is from 256 toward 1536, a large per cent of the observers changed the vowel from lower to higher as would be expected. A small per cent of the observers kept the same vowel or changed from a higher to lower. On the other hand when the change in interval is from 384 to 256, (a small change when compared with the preceding one) a high per cent of the observers recorded the vowel quality of the two tones as the same, a small per cent recorded a change from higher to lower (the true change) or from lower to higher. The number recording a change from higher to lower was of course greater than those recording the opposite change. These comparisons are made on the basis of the piano series. In the violin series, the results are practically the same except that in the change from 384 to 256, a higher per cent of the observers made a vowel change from higher to lower (the correct change) than those who kept the same vowel for both tones, as was the case in the piano series. This does not alter the conclusions to be drawn from these results however. The results simply mean that the observers discriminated a much greater change in vocality between tone 256 and tone 1536 than between tone 384 and 256.

Conclusions—The results of this study warrant the following conclusions:

1. There is unquestioned evidence that there is some degree of consistency in the vocality assigned to tones, whether produced by forks, piano or violin. This means that the vocality is a factor associated with pitch, but not one of absolute pitch.

2. The vowel quality ranges from the ū sound at the lower end up to the ī quality at the upper end of the scale, with a wide variety of vowel characters between the two, which become more and more like extremes as the upper or the lower end of the scale is reached.[7] The great majority of the observers agree as to the location of the ū and the ī vowel sounds.

3. The degree of consistency of the vowels assigned to the various vibration rates is greatest when the tones are produced by the tuning forks. This is probably due to the absence of overtones, the tones from the tuning forks being relatively much simpler or purer than the same tones produced by the piano or the violin. The vocality of any

[7] Titchener, E. B. 'Beginner's Psychology,' p. 52.

given tone depends not only upon the fundamental tone, but also upon the several overtones, and the intensity of each.

4. The results of this experiment agree with those of Köhler first, in that the vowel characters ū and ī are much the easiest to locate. Secondly, the location of the ō sound is somewhere near the same, 512 (Köhler located it between 520 and 530). Thirdly there is a gradual decrease of the ū quality and an increase of the ī quality as one proceeds from the lower to the upper end of the range. However these results do not agree with the assumption that the vowel character is dependent on frequency. Were this true the dominant vowel quality should be the same, whatever the instrument used. One of the principal differences obtained by using the several instruments, is the change in the number and intensity of the overtones present. We find that there is a shift in the location of some vowel characters with a change in instrument. Köhler reported that he had trouble with the observers, due to their unfamiliarity with the tuning forks. In the present experiments this was not true. Greater consistency was obtained with the tuning forks than with either of the instruments. Köhler suggests that the vowel character may be found not only at a given frequency, but at those frequencies which are in immediate proximity to it. He makes his theory of vowel character comparable in this wise to the resonance theory of Helmholtz.

No attempt was made in this study to point out a definite location for any given vowel sound, but the results do not point in that direction. The pitch of a vowel is not nearly so absolute as is assumed by Köhler. The order of vowels found as related to pitch range was indeed that obtained by Köhler, but the results would rather warrant the following assumption.

There is at the lower pitches a quality, which corresponds to the vowel ū of the speaking voice, and there is at the upper pitches a quality, which corresponds to the ī sound of the speaking voice. These might be referred to as a mellowness or a shrillness, as is done by Meyer, which we merely represent by means of the vowels. In between these limits there are many other characteristic sounds, which may resemble the ū or the ī or may be of such a character as to be identified with neither. These we indicate by means of other vowels which are formed in such manner as to have a mellowness or shrillness between the designated limits. The fact that, as Miller found, starting with the pure ō sound, one may by slowing down the movement of the graphophone produce a sound that becomes less like the ō and more like the ū, or by speeding up the movement, produce a sound that becomes less like the ō and more like the ī, does not necessarily imply that the vowel character is a function of the vibration rate of the fundamental only. The presence of upper overtones of high intensity will of necessity bring into the compound more of the ī element. This is shown particularly with the experiments on the violin tones. The presence of strong overtones whether high or low, will raise the vowel character. We accordingly find that when the tones are produced by tuning forks where the presence of overtones is comparatively unimportant, that the ū sound or quality predominates and that even with the higher pitches, the amount of the ī quality does not reach that of these same pitches when sounded from the piano or more especially the violin.

If a small mouthed bottle be held under a stream of water and allowed to fill, the tone produced by the water passing through the mouth rises in vowel quality as well as in pitch. This closing up of a larger cavity to a smaller produces the change from the mellow to the shrill quality, which we designate by means of the vowel equivalents. This is in some measure the same as what Watt[7] calls tone-volume, although he later defines vowels as "(partial) sounds of a somewhat indefinite pitch which lies about the point of range of pure tones of definite pitch where the greatest resemblance to these vowels is to be found." It seems, however, that it is not the slight variation in fundamental pitch that is so important, as the effect that the change in the intensity of the overtones has upon the whole, and thus upon the vowel character.

[7] Watt, Henry J., 'The Psychology of Sound,' p. 233.

RELIGIOUS BELIEF AND THE POPULATION QUESTION

By Wesley Raymond Wells, Colby College

In a previous article in this journal [1] I have spoken of the biological value of religious belief. Such considerations as were presented in that article ought to be supplemented by an account of one further situation, a very specific and concrete situation, in which religious belief manifests its biological value most strikingly. I refer to the correlation between religious belief and a relatively high birth-rate, especially in the case of belief of a somewhat legalistic sort such as is now best exemplified in Catholicism. This correlation is very significant. It shows that religious belief possesses survival-value of a high order.

The connection between religious belief and the birth-rate in ancient history is a well-known fact. Fustel de Coulanges[2] has pointed out how, in the patriarchal families of the ancient Greeks and Romans, religion required the continuity of the family. The fate of the ancestral spirits was believed to depend upon offerings made at their tombs by their descendants. Continuity of the family was required for the sake of the sustenance of the departed ancestors. Celibacy was an impiety, forbidden by religion and also by civil law when law arose out of religious requirements as something distinct from them. The ancient laws of Rome forbade celibacy.[3] The ancient Hindus had similar laws. For the Hindus as for the Greeks and Romans the extinction of a family caused the ruin of the family religion, and this was to be avoided by all means.[4] Among the ancient Hebrews also it was a fundamental religious duty to "be fruitful and multiply."

All this, however, is a matter of ancient history. It is more important to inquire about the relation between religious belief and increase of population in modern society.

[1] Vol. XXIX (1918), pp. 383-92.
[2] *The Ancient City*, (translation by Willard Small), Boston, 11th edition, 1901.
[3] See Cicero, *De Legibus*, iii, 2.
[4] See Fustel de Coulanges, *op. cit.*, p. 62.

In the first place, it seems a matter of common observation that, in general, the birth-rate is highest in families most influenced by religious belief. The reasons that might be assigned as the cause of this are various. In some cases the religiously inclined may feel a mystic obligation to rear a family. Fear of a concrete hell for those failing to fulfill their parental obligations may be the motivating power in other cases. Mr. Russell seems to incline to this explanation, and to think that the belief in hell-fire is thus biologically justified, when he says, "Men and women who can still believe the Catholic faith will have a biological advantage; gradually a race will grow up which will be impervious to all the assaults of reason, and will believe imperturbably that limitation of families leads to hell-fire"[5] A third possible explanation of the connection between a high birth-rate and religious belief is the partial identity of the parental instinct and the religious sentiment. According to McDougall,[6] the tender emotion, which is a correlate of the parental instinct, is a constituent also of the religious sentiment.

In the second place, statistical studies have supported the conclusions of casual observation by showing it to be a fact that, where religious belief thrives, there a relatively high birth-rate is generally to be found. The Italian premier Nitti[7] has called attention to this fact. Leroy-Beaulieu has made a statistical study of the situation, not only in France, but in various other countries as well. He has found a high birth-rate among the Spanish and Italian Catholics, and in those sections of France where the Catholic church is strong.[8] McDougall[9] accepts the contention of Benjamin Kidd[10] that one great influence of religious belief has been, and continues to be, its instrumentality as a support of the parental instinct against the tendency towards prudential limitation of the

[5] Bertrand Russell, "Marriage and the Population Question," *International Journal of Ethics*, Vol. XXVI (1915-16), p. 451.

[6] See Wm. McDougall, *Social Psychology*, pp. 66ff and Ch. XIII.

[7] Francesco S. Nitti, *Population and the Social Order* (translation), pp. 118-24.

[8] Paul Leroy-Beaulieu, *La Question de la Population*. See especially pp. 395-402. Leroy-Beaulieu gives the following explanation of the influence of religion, especially Catholicism, on population: "*La religion catholique, plus encore que toutes les autres enseigne la résignation à son sort, condamne l'égoïsme et déconseille l'ambition; c'est-à-dire, qu'elle exalte le sentiment qui tend à rende les familles nombreuses et qu'elle reprouve ceux qui tendent à diminuer le nombre des enfants.*" Pp. 397, 98).

[9] *Op. cit.*, p. 272.

[10] See Benjamin Kidd, *Social Evolution*, p. 295; also Ch. V.

birth-rate with developing intelligence. Sidney Webb[11] explains the high birth-rate among Catholics in England by reference to the fact that the Catholic church absolutely forbids any regulation of the marriage state. Of the distribution of this high birth-rate he says,[12] "It is significant that Ireland is the only part of the United Kingdom in which the birth-rate has not declined; that in Ireland itself it has declined a little in semi-Protestant Belfast, and not at all in Roman Catholic Dublin; and that in the towns of Great Britain the decline is least in Liverpool, Salford, Manchester, and Glasgow—towns in which the proportion of Roman Catholics is considerable." Webb finds that among the metropolitan boroughs the highest birth-rate is "in those boroughs in which the Irish Roman Catholics (and the Jews who, in this respect, are in the same position) are most numerous."[13] Many economists have tried to correlate high and low birth-rates with conditions of poverty and wealth, but Webb shows that there is a closer correlation with religious belief than with economic conditions. As a matter of fact, however, poverty and religious belief are frequently found together since many people find compensation in religion for the lack of the material satisfactions of life.

For a most complete and convincing array of statistics upon this subject, Mr. Meyrick Booth's article, "Religious Belief as Affecting the Growth of Population,"[14] should be consulted. Booth shows, for example, that, according to the Catholic Year Book for 1914, the birth-rate per 1000 of Roman Catholics in Great Britain was 38.6, while the average rate for the whole population was but 24.0. In the United States, among those states which, according to the religious census of 1906, had a small population of Catholics, the birth-rate was low, averaging 15 or less per 1000. On the other hand, in five states with a large proportion of Catholics, the birth-rate averaged about 23 or 24 per 1000.[15]

There may be a temperamental basis for susceptibility to religious belief, and temperament is hereditary. As McDougall says,[16] basing his conclusions on studies made by Karl

[11] *The Decline of the Birth-Rate*, Fabian Tract, No. 131, 1907.
[12] *Ibid.*, p. 9.
[13] *Ibid.*, p. 9.
[14] In the *Hibbert Journal*, Vol. XIII (1914-15) pp. 138-54.
[15] Booth's article is a careful study of the situation, and gives many tables of statistics, which should be consulted by anyone who is not convinced of the importance of religious belief as influencing population increase.
[16] *Op. cit.*, p. 273.

Pearson,[17] about one-half of each generation is recruited from one-quarter of the preceding generation. The quarter with highest birth-rate is, as I have shown, the most religious portion of society. So far, then, as there is a temperamental basis for religious belief, and so far as temperament is hereditary, the religious temperament, and consequently religious belief, can not die out while the birth-rate is highest among those possessing the requisite temperament.

The correlation between religious belief and a relatively high birth-rate is most conclusive evidence of the biological value of religious belief. If unbelievers are not so much interested as others in founding families, then their unbelief perishes with them; while religious belief springs up in each new generation from a perennial source of vital human need.

[17] See Karl Pearson, *Chances of Death.*

A NOTE ON PEN-LAPSES, INITIATED VISUALLY

By June E. Downey

The recent analysis by Dr. Roback[1] of a number of pen lapses, and, in particular, his observations relative to the possibility of a pen lapse being set off by the visual perception of a word already written leads me to record a number of self-observations made on pen-lapses occurring spontaneously during rapid composition.

My records were made immediately on recognition of the lapse. It was usually possible to call back the graphic cue and in other respects to snapshot the process.

As a preliminary to the report I may say that in composition my cue is very distinctly acoustic-vocal-motor. There are present, in addition, hand-motor sensations, since the writing movements involve considerable tension; and, also, a high degree of visual consciousness of what the written product looks like. Only under very great absorption in the content do I lose this consciousness of visual supervision. One consequence of this is that pen lapses rarely get past the visual censor; incipient lapses are apt to be detected in the making. Another consequence is, I suspect, the initiation of an occasional lapse through visual suggestion. I select the following cases for report, quoting directly with a few additions in parentheses, from notes made at the time the lapse occurred.

1. "Started to write 'coats' for 'goats.' After writing 'co,' I stopped and wondered what was wrong. The acoustic-motor cue was unambiguously 'goats,' but, visually, the 'co,' with a vague visual continuation of 'ats' seemed just as meaningful. There was considerable hesitation before correction was made....Definite split between the auditory-vocal-motor cue and the visual report on the process. Was the origin of the lapse grapho-motor in nature? (A more probable cause would seem to be the similarity in sound between the 'c' and the 'g.').....Noticed later that the word 'color' occurred in the line above, three inches to the left. Perhaps the sight of this 'c' suggested the lapse. 'Color-blindness' was the topic of the paragraph, so that 'C' may well have persisted in consciousness."

2. "Wrote 'world(s)' for 'words.' This lapse was corrected before 's' was completed; 's' was on the tip of the pen when writing was halted and the correction made..... This lapse is interesting in that it parallels another lapse recorded the same day, ('coats' for 'goats'). The word 'world' occurred in the preceding line, two inches to the left of the place where the lapse occurred."

3. "The auditory-vocal-motor cue was, clearly, 'give' but I wrote 'keep,' recognizing the lapse as soon as the word was written. Was unable to see any reason for such a lapse, then, recalling my observation of a month before, looked up and to the left—sure enough! there was the word 'Keeps' first word in second line above, one and five-eighths of an inch to the left."

[1] This Journal, 1919, 30, pp. 274-290.

4. "Was taking notes on book I was reading. Wrote 'See Galton p. 20g,' for 'See Galton, p. 209.' (Both G's were written in capital form but with a long down loop.) Distance from the 'G' in Galton to the interpolated 'G' was one and three-fourths inch. The graphic cue was largely visual, the number 209 as visualized on the page; no lip-motor consciousness. Possibly the lapse was due to grapho-motor perseveration, long down stroke on the 'g' (initial stroke on '9' and on 'g' being similar); possibly there was a visual suggestion operating from the peripherally seen G."

5. "Wrote on the margin of a theme I was correcting, 'Collected' for 'Connected.' Sentence read, 'Should be parallel in form and connected.' It was about two inches from the first 'l' in 'parallel' to 'l' as written in 'collected.'

6. "Was intending to write 'On the other hand;' I wrote, instead. 'On the who(le).' The auditory cue was 'hand' but perhaps the word 'whole' was there in the margin. Lapse was immediately recognized visually. After writing an account of the lapse I examined the writing tablet to see if a 'w' had occurred in the preceding line. The word 'what' was found in the line above, about one-half an inch to the right of where the lapse occurred."

7. "Wrote in a letter 'I've been hoping to see your article in print ere this. Of course it's hop(ing).' The verbal cue was definitely 'Of course it's worthwhile.' The lapse was recognized visually at once. It would seem induced by visual suggestion. In the manuscript the first 'hoping' is about two inches to the left and a half inch above the second 'hoping'."

Other sorts of lapses were of course recorded. Perhaps most interesting of all are those which result in a hybrid word, due to coalescence, of the intended and of the automatic word. Sometimes introspection reveals an inchoate or confused meaning-consciousness, which is highly instructive. One or two examples may be given.

8. "Wrote 'Emotent,' for 'Emotion constitutes the content.' This lapse was recognized as soon as written. (No analysis)."

9. "Wrote, 'Going to the door I saw Dig outstretched on the back porch.' There was a conflict in writing-cue between 'Nig' (name of our cat) and 'Dick' (name of our dog); 'Dick' prevailed as cue. It was not until rereading my notes next day that I perceived the coalescence of the two cues. This lapse escaped the visual censor."

10. "In correcting a student's theme I started to write 'please' for believe.' Recognized this error when I started on the below-line stroke of 'p' (a visual and motor recognition). Auditory-vocal-motor cue was a blurred 'b-l-l.' I had just been thinking out instructionss to class. I intended to say. 'Please save this theme.' There was a perseveration of meaning which was influential, together with the similarity in sound between 'b' and 'p,' in causing the lapse."

None of these lapses, except possibly 3 and 7 lend themselves to a Freudian interpretation. That Freudian lapses, motivated by unconscious complexes do occur I am convinced. I have, for example, written 'toxin' when I meant 'tocsin' and the like, but such lapses seem the exception, not the rule.

APPARATUS NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Protractor for Color-Mixing. The annular brass protractor for color-mixing, which cost over six dollars before the war, costs considerably more now. Although it is very satisfactory in use, it is expensive when purchased in quantity for laboratory courses where several experiments in color-mixing go on simultaneously.

In the Clark Laboratory we have duplicated this protractor in a line engraving that can be printed on cardboard. The cut prints an annular ring, 13 cm. outside diam. and 10 cm. inside diam, with a scale of single degrees marked on both outside and inside circumferences. The center of the print is marked with concentric rings that aid in punching a hole for the disk-cutter. The outer circumference is cut entirely about by the disk-cutter. In cutting the inner circumference a bar (2 cm. wide) is left to stretch across the circle and to support in the center another annular ring that fits over the spindle of the mixer and centers the proctractor. The bar and central ring could be made of metal and the cardboard scale cemented thereto, but it seems to be satisfactory simply to cut the whole protractor out of the cardboard.

When printed on 4-ply board this protractor is stiff enough to stand ordinary usage, and thin enough to permit accurate reading at the edge. It is easy to read half-degrees on both inner and outer scales.

The Clark Laboratory will be glad to supply psychologists with these protractors, printed on cardboard but uncut, at cost which is at present about two cents apiece.

Artificial Daylight. Although the type C-2 Mazda lights do not give an exact artificial daylight, they provide a constant illumination that is more satisfactory when colored papers are used (demonstrations, the drill experiments in color, color preferences, *etc.*) than is uncontrolled daylight.[1] The uneven illumination can be avoided by having the lamps frosted at an electrical store. The frosting reduces the illumination scarcely at all; and it makes the lamp less uncomfortable in direct view.

E. G. B.

Exposure Apparatus for Memory Experiments. An inexpensive form of apparatus for successive exposures, devised by the late Professor J. W. Baird, has been found to be of service for presenting the materials of memory experiments in elementary laboratory classes and in group demonstrations. Nonsense syllables of black gummed letters, 1 in. high by 0.8 in. wide, are pasted on cardboards, 7 in. by 5½ in. These cards are then made into a booklet by the use of gummed cloth tape.[2] The tape is stuck on the bottom edges of the back of one card and of the front of the card behind it. In the same manner, all the cards are successively fastened into a booklet. A blank card is put at the front of each booklet. It is convenient in pasting the letters

[1] Cf. G. J. Rich, this JOURNAL, XXX, 1919, 313-315.
[2] Cf. E. O. Finkenbinder, this JOURNAL, XXIV, 1913, 12.

on the cards to use a cardboard form with a rectangle of the extent to be covered by the letters of the nonsense syllable cut from the center. This form is placed over the card on which the letters are to be pasted, and assures uniformity in both the placement and the spacing of the letters. The cards are then notched or 'indexed' across the upper edge so that the experimenter can readily drop one page at a time. When the last card has been dropped, the booklet lies face downward on the table. The experimenter can then pick it up and give a second exposure of the series by again dropping the pages one at a time.

The booklet can also be made with a blank card at the end and the cards notched or 'indexed' on the side.[3] In this case the last blank page is attached by thumb tacks to an upright blackened board, in which a narrow slot of a size corresponding to the notching on the edge of the booklet is cut. This booklet is held up in position by a small metal hook which is passed through the slot in the board; and the exposures are made by moving the metal hook upward over the successive excisions in the cards. This form of booklet has an advantage over the first in that the observer by its use is less apt to be distracted by the sight of the hands of the experimenter S. W. F.

[3] Cf. S. C. Fisher, *Psychol. Mono.* xxi., (Whole No. 90), 1916, 35.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

By E. B. Titchener

1. Electromagnetic Control of Stop-Watch

This little instrument serves a twofold purpose: it safeguards the stop-watch against damage by careless handling, and it enables *E* or *O* to actuate the watch at a distance. The control was suggested to me by Jastrow's "Electric Attachment for the Stop Watch," which is figured in the catalogue of the Garden City Model Works, 1894, no. 11. It consists essentially of an electromagnet with pivoted armature, reinforced by a coiled spring. Watch and magnet are cased in a house of cast metal, with round window for the watch-face; and the clips that hold warch to stand, the pin that governs the tension of the spring, and the actuating button of the armature are all adjustable, so that the control may be applied to any make of watch. If the spring is set at the right degree of tension, and if the armature button is centred on the stem of the watch and moves in accurate alignment with the push-pin, the arrangement works—with the ordinary cheap stop-watch—on a single dry cell; a refractory watch may need two cells. The click at make, which is sometimes disturbing, may be lessened by papering the face of the armature.

The control is sold by the C. H. Stoelting Co., 3047 Carroll Avenue, Chicago, Ill.

2. Curve Tracer

It has always been difficult to draw accurately arcs of circles of large radius. Since these curves are variously useful in laboratory work, I have asked the C. H. Stoelting Co. to manufacture Weitzenböck's device (described by K. Bühler *Die Gestaltwahrnehmungen*, i. 1913, 72). The model supplied to the Cornell Laboratory has been subjected to preliminary tests, and appears to work as accurately as the Wolz original.

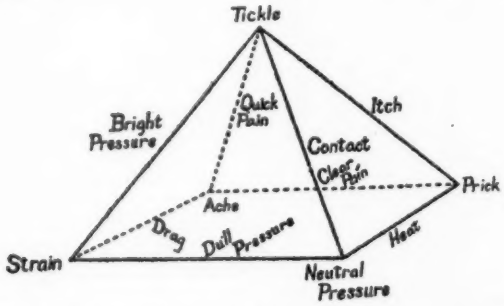
3. Models for the Demonstration of Sensory Qualities

There is nothing like a geometrical model for bringing home to beginners in psychology the nature of the qualitative series

and the positional character of the qualitative attributes. Some years ago I described a demonstrational color pyramid (this JOURNAL, XX., 1909, 15 ff.). I wish now to say a word regarding a set of four simpler (frame or outline) models that I have found useful for demonstration in introductory courses. Dimensions do not matter, though on general principles the larger the models are made, the better.

The frames are built of light metal rods, blackened, whose ends are held together by cubical blocks of blackened wood. The blocks are bored to receive the posts of paper flags, which show the initials (or other symbols) of the terminal qualities. The rods further carry one or more sliding blocks for the similar indication of intermediate qualities. The frames thus constructed will stand on any face; the flags may be interchanged for demonstrational purposes within the given model; and wires may be stretched as wanted across surface-planes and interiors.

The four models are the color double pyramid; the taste tetrahedron and the smell prism (H. Henning, *Zeits. f. Psych.*, lxxiii., 1915, 254 f.; lxxiv., 1916, 212); and a touch pyramid, here figured.



I need hardly say that several of my determinations of the touch qualities are tentative, and that I regard their arrangement at best as only a rough approximation to the truth. I publish the figure because I think it marks the direction of recent work; because it may perhaps serve as the starting-point of new experiments; and also for the more personal reason that I have shown the model in my lectures of the past two years, and that the observers in certain investigations shortly to be published from the Cornell Laboratory have therefore been familiar with its terms and series.

For the place assigned to heat, see F. Cutolo, this JOURNAL, xxix., 1918, 445, 447 f.; for what I have called 'quick' pain, see E. Becher, *Arch. f. d. ges. Psych.*, xxxiv., 1915, 205. Warmth and cold for the present baffle me completely; I cannot bring them into qualitative relation with the terms of the figure; but the fact that they are left outstanding—I admit that it is a real difficulty—cannot of itself discredit the figure, and can still less discredit the underlying idea. Finally, auditory sensations may be systematized in various ways; and I have no doubt that some sort of closed figure will presently be found adequate to them. So long, however, as tonality and vocality are in debate, it is hardly safe to propose an arrangement of qualities.

4. SEWING MACHINE MOTOR

I am indebted to Dr. E. G. Boring, now of Clark University, for calling my attention to this motor, which is sold under the name "Sew E-Z" or "Home Motor," by the Hamilton Beach Manufacturing Company, Racine, Wis. The motor is intended for use with sewing machines, and in other household operations. For color mixing in the laboratory it has various merits; it requires practically no attention and stands hard usage. The motor is strongly cased, and its oil-cups are accessible. It will operate on either alternating or direct 110-volt current. It runs in both directions: the direction is reversed, very simply, by loosening a set-screw and shifting the brushes.

The motor is provided with a 6-speed rheostat, operated against a spring by a foot-treadle. The rheostat can be removed from its attachments, and turned by hand on the table. Thus separated, it is convenient and compact: 4 cm. high by 7 cm. diam. The motor itself with its base goes within a 15 cm. cube. The motor is light in weight, and though it is steady enough for ordinary color mixing must be clamped to the table if the discs are unbalanced (as in photometry by the Kirschmann method).

The motor has very little power, but great speed. Boring's tests show that on no-load the six points of the rheostat give about 2000, 4500, 7000, 9500, 12000 and 13000 r. p. m. A load of discs largely reduces this speed; but 20 cm. discs can be rotated on the fourth rheostat point at a speed of 4000 r. p. m., which is ample to prevent flicker.

The Cornell Laboratory has had the sewing machine motor in steady use since 1916, and the Clark Laboratory has recently adopted it. The current price (without discounts) of motor, rheostat, connecting cord and pulley is $18.50.

NOTE ON THE EXPERIMENTAL STUDY OF ATTENTION

By K. M. Dallenbach

It seems necessary to reply briefly to two recent criticisms of my work on the experimental investigation of attention (this Journal, xxiv., 1913, 465 ff.; xxvii., 1916, 443 ff.).

(1) With the remarks of K. Koffka (*Zeits. f. Psych.*, lxxiii., 1915, 281 f.) I am, if I understand them aright, in substantial agreement. But that, of course, is to say that I regard them as irrelevant, in so far as they are intended for a criticism of my method. Koffka, from the standpoint of his own theory of attention, attempts an analysis of the conditions of attention operative in my experiments. The conditions of observable clearness, however, mattered as little to my observers as would the conditions of the appearance of two greys in an experiment on the discrimination of brightness. We, by hypothesis, were observing a sensory attribute of the intensive sort, and were trying to correlate the quantitative changes of this attribute with objectively measurable variations. Clearness was the starting point, not the end-point, of the investigation.[1]

(2) H. Henning (*ibid.*, lxxxii., 1919, 245) adopts a more polemical tone, and is led thereby into several inaccuracies of statement. These are of minor importance: the serious thing is that he has failed to appreciate the significance, in this kind of work, of the 'wrong' judgments, and the interrelation of the Single Task and Double Task methods. He wholly misses the central fact of my results (though I naturally laid special stress upon it), that the curves of the 'right' judgments are skewed toward the higher, those of the 'wrong' judgments toward the lower degrees of observed clearness. A reader who should take Henning's review as adequate to my paper would, therefore, be misled.

[1] Britz' *Theoretische u. experimentelle Untersuchung ueber d. psychol. Begriff d. Klarheit* (1913) has been discussed by Titchener (*Psych. Rev.*, xxiv, 1917, 43 ff.). I have myself repeated and extended Britz' experiments, and give a critical account of the results in the *Journ. Exper Psych.*

BOOK REVIEW

Anpassungstheorie des Empfindungsvorganges. By JULIUS PIKLER, Leipzig, Johann Ambrosius Barth, 1919.

1. *Hypothesenfreie Theorie der Gegenfarben.* 104 p.
2. *Theorie der Konsonanz und Dissonanz.* 34 p.

These two contributions to the study of visual and auditory theory clearly take root in a more comprehensive doctrine of sensory phenomena in general, which the author published in 1917 (*Sinnesphysiologische Untersuchungen*, Leipzig, 516 p.). It would seem that this latter doctrine has indelibly colored the University of Budapest professor's outlook upon data presented by the separate sense departments. The first part of the present monograph (published separately) treats of visual theory. Hering's views on this subject come in for the brunt of adverse criticism.

Bk and *W* are at opposite extremes of an uniform scale of brightnesses which passes through a central or normal point of middle grey. The somatic basis for the sensation of *Bk* is found in the complete absence of that process which conditions *W*, and the basis for greys in the varying degrees of partial absence of this process. The psychological antagonistic positivity of *Bk* derives from the representation in consciousness of a normal middle point in this process to which the organism has become *eingestellt*, and as a result of which deviations in either direction are perceived. The seeing of *Bk*, *W*, and greys, then, is conditioned by corrective adaptation processes with respect to the maintenance of an autonomous middle grey equilibrium. These adaptive corrections tend to raise or lower the point of equilibrium, from which in turn it follows that these same corrective processes in the direction of increase or decrease become greater or less,—whence the phenomena of simultaneous contrast. Positive after-images are due to the continuation of the adaptation process after cessation of stimulus; negative after-images to the excess of adaptation process after prolonged stimulation which is adequate to a new stimulus in the direction of greatest opposition to the previous sensation.

For the phenomena of color tone, Pikler elaborates and expands the above theory. In rough, his position is somewhat as follows: Colors distribute themselves along a scale in accordance with brightness values similar to the achromatic series. Each hue possesses a constant and "specific" tint. *B* is at the bottom of the brightness scale, *R* and *G* in the middle, and *Y* at the top. The following diagram represents the relation of the achromatic to the chromatic scale:

	Dark grey	Grey	Light grey	
Black				White
	Red blue	Red	Red yellow	
Blue				Yellow
	Green blue	Green	Green yellow	

The phenomena of color vision are explicable only in the light of their brightness components, since the somatic processes underlying

the latter are in reality the basis of the former as well. If this be true, then it is clear that *B* and *Y*, e. g., are antagonistic by virtue of their opposite location on the brightness scale. The same explanation given above for contrast, after-images, etc., is applicable to similar hue phenomena.

Part 2 is an extension of the author's general adaptation theory of sensation to the field of audition. Strangely enough, consonance and dissonance is the only subject treated!

The hearing of tonal intervals is fundamentally the perception of geometrical relations. We perceive in the octave the geometrical relation 1:2. The upper tone is the same as the lower, only doubled. This is explained in terms of the adaptation theory by the arousal of a sensation process by the lower tone of the octave, and a corrective adaptation sensation process just double in size by the higher tone. In the case of the fifth, we have the geometrical perception of the higher tone half double the lower, i. e., we perceive the ratio $1 : 1+1/2$, not 2:3. With the major third the upper tone is perceived to be the same as the lower, only one-fourth doubled, i. e., $1:1+1/4$, instead of 4:5. These intervals are the only ones characterized by sameness of components, whence derives their consonance. All other intervals are perceived arithmetically, and the component tones are heard unlike and dissonant.

It is rather difficult to escape the conviction that facts of audition have here been forced into an hypothesis by dint of uncritical analogy with space perception. Remarks Pikler, "Der Gedanke, dass es auf dem Tonhoehengebiete geometrische Verhaeltniswahrnehmungen gibt, ist, soweit ich sehe, bisher niemandem gekommen." How stupid psychology has been!

C. C. PRATT.

BOOK NOTES

An Introduction to Social Psychology. By WILLIAM MCDOUGALL. Lond., Methuen and Co., 1920. 459 p.

This is the fourteenth edition of this author's most important work. He has tried to deal with a difficult branch of psychology in a way to make it intelligible to the cultivated reader and without implying any previous familiarity with psychological treatises on his part. There is considerable elaboration in Chapter 3 of a principle formerly enunciated, namely, that all emotion is the affective aspect of instinctive process. He combats the current view that imitation is to be ascribed to an instinct, elaborates his conception of sentiment in a somewhat new way, analyzes the principal complex emotions in the light of the conception of the principle laid down in an earlier chapter on instinct and instinctive process, and finally applies the results of his theories to the description of the organization of the life of emotion and impulse.

The Problem of the Nervous Child. By Mrs. ELIDA EVANS. (With an Introduction by Dr.C. G. JUNG of Zurich.) N. Y., Dodd, Mead, 1920. 289 p.

After years of study of nervous children and parents by the psychoanalytic methods of Dr. Jung, the author here presents an account of the part played in the present life of the adult by mentally bad environment during childhood. The book has many practical illustrations of mistakes, and the cases showing how present nervous trouble in adults is directly traceable to childhood gives the work a certain interest for others than parents and teachers.

Lehrbuch der Logik. By TH. ZIEHEN. Bonn, A. Marcus and E. Weber, 1920. 845 p.

This comprehensive work is connected with the author's psychology and theory of knowledge but independent of them, as the work in pure logic should be. He deals first with the problem, then with the general theory of logic, with its relations to epistemology and psychology, the doctrine of judgment, and finally states his inferences and conclusions.

The Ground and Goal of Human Life. By CHARLES G. SHAW. N. Y. Univ. Press, 1919. 592 p.

The author here attempts a "treaty of peace" between the forces of individualism and those of social thought. In Book I, The Ground of Life in Nature, he deals with the self as thinker, the empirical ego, the surrender to naturalism; Part II of this first division discusses the struggle for the joy, worth, and truth of life. Book II treats of the Goal of Life in Society, including the transvaluation of self and society, the repudiation of sociality, etc. Book III, entitled The Higher Synthesis, takes up the question of the joy, worth, and truth of life in the world-whole.

Life Movements in Plants. By Sir J. C. Bose. Calcutta, Bengal Govt. Press, 1919. 597 p.

This extremely able student of the physiology of plants here sums up his recent experiments, with considerable reference to those that have preceded. It is interesting to note that he has founded an Indian school of physiology, which is well subsidized and of which his country should be very proud. His ingenuity, originality, and independence of thought have enabled him to carry the subject of physiology of plants very distinctly beyond that which any of the many pioneers have attempted before.

Education in War and Peace. By Stewart Paton. N. Y., Paul B. Hoeber, 1920. 106 p.

Peace, no less than war, produces shell-shock, the symptoms of which interfere with individual efficiency, happiness, and social progress. This work calls attention to the urgent necessity of making adequate provision in our universities for training investigators competent to undertake the solution of the vital important educational problems now confronting civilization. The chapters are: Human Behavior in War and Peace, War and Education, and The Psychiatric Clinic and the Community.

A Beginner's History of Philosophy. By Herbert Ernest Cushman. Bost., Houghton Mifflin, 1919. 407 p.

We have here Volume II, entitled "Modern Philosophy" (1453 to the present time). The author's work is intended as a textbook for sketch-courses in the history of philosophy and is written for the student rather than for the teacher. It is based on a background of geography and literary and political history, and its aim is to arrange and organize the material of the history of philosophy for the beginner. This volume begins with the causes of the decay of the civilization of the Middle Ages and ends with the philosophy of the nineteenth century.

Studies in Contemporary Metaphysics. By R. F. Alfred Hoernlé. N. Y., Harcourt, Brace and Howe, 1920. 314 p.

These studies are described as "chips from a metaphysician's workshop" or "blocks hewn out experimentally in the effort after a systematic synthesis." They are like the painter's sketches or the sculptor's modelling in clay, which precede the finished work. The book consists of ten papers with a common animus but more or less independent of one another. He deals with the scientific method in philosophy, the philosophy of Nature, "doubting the reality of the world of sense," mechanism and vitalism, theories of mind, the self in self-consciousness, and religion and its philosophy.

The Intellectuals and the Wage Workers. By Herbert Ellsworth Cory. N. Y., The Sunwise Turn, 1919. 273 p.

The chapters here are: Equality, Proletarianism, Religion, Criticism, History and Freedom, Liberty, The Class Struggle and Fraternity, and Education: A Program for the American University. It is dedicated to Carleton H. Parker, whose name suggests the spirit in which the author writes.

Spiritual Pluralism and Recent Philosophy.. By C. A. RICHARDSON. Cambridge, The Univ. Press, 1919. 335 p.

This work is dedicated to James Ward from the inspiration of whose teaching it derives its main theme. The author's purpose is to present and defend the theory that the texture of the Universe is through-and-through spiritual but he admits that he has been influenced by the Neo-Realists of America and by Bertrand Russell.

Pagan and Christian Creeds. By EDWARD CARPENTER. N. Y., Harcourt, Brace, and Howe, 1920. 319 p.

In this book the author of "Civilization: Its Cause and Cure" propounds a new theory concerning the origin of religious rites and ceremonies to prove that Pagan and Christian cults had the same source. He deals with the astronomical and magical aspects of the subject, the rites of initiation, redemption, sex-taboos, ritual dancing, legends of the Golden Age, the Eleusinian Mysteries, and the progress of Christian thought.

La Meccanica del Cervello. By LEONARDO BIANCHI. Rome, Bocca, 1920. 425 p.

This work sums up the extended investigations of the author on the mechanics of the cerebellum.

Aphasia and Associated Speech Problems. By MICHAEL OSNATO. N. Y., Paul B. Hoeber, 1920. 191 p.

This is a summary of the subject up to date.

COMMUNICATION—PSYCHOLOGICAL TERMINOLOGY

The Committee on Terminology of the American Psychological Association is taking up for examination terms in the fields of Sensation and Cognition. Psychologists interested in the precise use of terms are invited to assist the Committee in its work by calling the chairman's attention to—

(1) Psychological terms used with two or more different meanings (whether distinguished or not), and terms used indefinitely or ambiguously in contemporary writings.

(2) Pairs or groups of terms which lead to confusion when used interchangeably.

(3) Foreign terms needing definition or translation.

(4) Books and articles containing systematic lists of cognate terms, or discussions of ambiguous terms. (Full references desired.)

It is a matter of prime importance in any science to clear up double meanings and imperfect synonyms. The word *feeling* is used in standard psychological works with several different meanings. The words *intellect* and *intelligence* are used by some writers interchangeably, while others draw a sharp distinction between them. There are many instances in the literature of both kinds of confusion.

The Committee wishes to include a large number of such terms in its next report, either defining and distinguishing them or citing discussions in easily accessible sources. This list will not be confined to sensation and cognition, but will cover the entire field of psychology. Will readers of this magazine assist the Committee to make the list fairly complete?

HOWARD C. WARREN, Chairman,
Princeton University.